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Cover illustration. Nativist riot in Philadelphia, 1844. (Detail from fig. 8, p. 391.)

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Rioting in Its Jacksonian Setting

DAVID GRIMSTED

"AMERICANS HAVE ALWAYS BEEN a benevolent people." As the Romantics argued the kinship of madness and genius, so it is difficult clearly to segregate student inspiration from imbecility. And if not the student who wrote the comment in that eternal source of peculiar wisdom, last year's exam books, perhaps Providence acting through the student spoke suggestively, especially considering the root of the word: if *bene-volo* means to will good, "benevolent" would suggest the willing of good in notably vigorous form. And here perhaps lies a clue to some of the paradoxes of both the origin and significance of social violence in its American contexts.¹

Social violence has obvious roots in both the psychology of its participants and their socioeconomic situation, and analysts of crowd behavior understandably have concentrated their examinations around such causes. Yet the extent, nature, and direction of mob violence depend equally on shared cultural assumptions about the nature of power and law, and the relation of the individual and the group to them.² For the Jacksonian period, the diversity of type and circumstance of riot offers presumptive evidence that social violence owed less to local and particular grievances than to widely held assumptions and attitudes about the relation of the individual

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¹ As recent popular books have stressed the peculiar violence of American society, more scholarly ones have suggested the limits as well as the extent of disruptiveness in the United States. Irving J. Sloan, *Our Violent Past* (New York, 1970); David Abrahamsen, *Our Violent Society* (New York, 1970); Ovid Demaris, *America the Violent* (New York, 1970); Hugh Davis Graham and Ted Robert Gurr, eds., *The History of Violence in America* (New York, 1969); Richard Hofstadter, introd. to Hofstadter and Michael Wallace, eds., *Violence in America: A Documentary History* (New York, 1970).

² Ted Robert Gurr, who has constructed the most complex and satisfactory sociological model for violence, argues that ideological sanctions for violence are important, but in "a secondary, rationalizing" way. Because "relative deprivation" is his key concept, Gurr claims that social tensions develop first, and from these grow intellectual sanctions for violence. One could argue equally well that deprivation relative to something or other is always with us, and violence depends more on ideological or cultural channeling. *Why Men Rebel* (Princeton, 1970). 13-15, 155-231.

to social control. Only a cause that was "general in its operation," wrote a Baltimore newspaper, could explain the variously directed outbursts of social violence in the mid-1830s.³

Historians of eighteenth-century America have shown that mobs in that period functioned more as an accepted part of the political structure than an attack on it, largely because authorities unofficially recognized their legitimacy so long as they acted within certain bounds.⁴ This reflected in part English preference for granting the lower classes occasional informal sway to giving them any established influence on government and in part colonial willingness to use the mob to make imperial authorities heed local interests. With the achievement of independence both of these justifications of the mob were undercut. Power was no longer imperially centered, nor were there large groups of white males denied a measure of political influence through established channels. Royall Tyler's play, *The Contrast*, written partly as a Federalist political document in 1787, marked the change clearly. Tyler, who had been active in putting down Shays's Rebellion, has his likably naive American democrat, Jonathan, admit that he was talked out of siding with Shays only by the natural aristocrat, Colonel Manly, who explained to him, "It was a burning shame for the true blue Bunker Hill sons of liberty, who had fought Governor Hutchinson, Lord North, and the Devil to have any hand in kicking up a cursed dust against the government which we had, every mother's son of us, a hand in making."⁵ The strong though unvindictive action of the authorities toward Shays's and Fries's and the Whisky rebellions—even the fact that these incidents were labeled "rebellions"—made clear that the eighteenth-century role of the riotous crowd had ended.

The United States in the first quarter of the nineteenth century was relatively free of internal group violence, but in the 1830s riot once again became frequent. Between 1828 and 1833 there were some twenty incidents of riot, in 1834 at least sixteen riots took place, and in 1835 the number increased to thirty-seven, most of them concentrated in the summer and fall of that year.⁶ The *Philadelphia National Gazette* echoed the sentiments of many when it wrote in August 1835, "The horrible fact is staring us in the face, that, whenever the fury or the cupidity of the mob is excited,

³ *Baltimore Republican*, Aug. 20, 1835.

⁴ Pauline Maier, "Popular Uprisings and Civil Authority in Eighteenth-Century America," *William and Mary Quarterly*, ser. 3, vol. 27 (1970): 3-35; Maier, "The Charleston Mob and the Evolution of Popular Politics in Revolutionary South Carolina, 1765-1784," *Perspectives in American History*, 4 (1970): 173-96; Gordon Wood, "A Note on Mobs in the American Revolution," *William and Mary Quarterly*, ser. 3, vol. 23 (1966): 635-42; William Ander Smith, "Anglo-American Society and the Mob, 1740-1775" (Ph.D. dissertation, Claremont Graduate School, 1965).

⁵ Royall Tyler, *The Contrast* (Boston, 1920), 55-56.

⁶ Incidents of riot were compiled from secondary sources, particularly local histories, from a complete reading of *Niles' Register* and the *National Intelligencer*, and from scattered reading in other newspapers, journals, and manuscripts.



Death of the Prophet.

Fig. 1. Mormon founder Joseph Smith and his brother Hyrum were shot to death by a mob in Carthage, Illinois, on June 27, 1844. After the Mormons mobbed a dissident press in Nauvoo, the Smiths were arrested. Governor Thomas Ford left them in the Carthage jail under the protection of a troop of militia who, after mock defense of their prisoners, cooperated with the mob in the murders. The illustration is from Henry Mayhew, *History of the Mormons or Latter-Day Saints with Memoirs of the Life and Death of Joseph Smith, the "American Mahomet"* (Auburn, N.Y., 1853).

they can gratify their lawless appetites almost with impunity; and it is wonderful with all the evidence of the facts that have been furnished in such abundance, to behold the degree of supineness that exists."⁷ Never again in the antebellum period was rioting this concentrated, but it remained a regular social phenomenon to be accepted with a degree of "supineness." Some of these incidents were in result minor—for example, the anti-Garrison mob, which resulted in one torn coat and one broken sign—but property damage was often extensive, and numerous lives were lost. By 1835 at least 61 people had been killed in riot; by 1840 that figure goes above 125, and the worst destructiveness was yet to come. Certainly over one thousand people were killed in antebellum riots, and the draft riots of 1863 added probably about another thousand to that toll.⁸ Even when the riots were comparatively undestructive, they revealed major tensions in the society: ethnic hatreds; religious animosities; class tensions; racial prejudice; economic grievances; moral fears over drinking, gaming, and prostitution; political struggles; the albatross of slavery.

That these incidents had been largely forgotten until the last few years tells us something, as current violence experts have said, about the way Americans have accentuated the positive in their past. More important, it reflects the way in which American democracy has been able to absorb quantities of violence in its structure without fundamentally shaking it.⁹ Historians have neglected the topic in large part because people so quickly forgot about the incidents. At times between 1834 and 1837 there was in some men's minds a sense of a real possibility of social disintegration, but even during these years there was always a quick return to placidity after the outbreaks. And as resort to violence proved not a steadily spreading disease but a kind of periodic social virus, unpleasant perhaps but also unthreatening to the social organism, fearful responses became shorter and more ritualistic. For a day or two after a riot some papers explored the specific situation and the general problem; a week later, unless a trial or coroner's inquest reawoke interest, it would be publicly forgotten. Riot had regained its eighteenth-century status as a frequent and tacitly accepted if not approved mode of behavior.

Acquiescence in riot owed much to the fact that rioting was not basically an attack on the social system itself. Francis Grund, the Jacksonian publicist, wrote that lynch law, a term often used interchangeably with riot in these years, "is not properly speaking an opposition to the established laws of the country . . . , but rather . . . a supplement to them—as a species of

⁷ *Philadelphia National Gazette*, Aug. 11, 1835.

⁸ Accounts about the number of deaths in particular incidents are frequently vague, unconvincing, or contradictory, but these figures are minimal, except possibly for the New York Draft Riot.

⁹ Richard E. Rubenstein, *Rebels in Eden* (Boston, 1970); Hugh Davis Graham, "The Paradox of American Violence: A Historical Approach," *Annals of the American Academy of Political and Social Science*, 391 (1970): 75-82.

common law.”¹⁰ A working definition of riot would be those incidents where a number of people group together to enforce their will immediately, by threatening or perpetrating injury to people or property outside of legal procedures but without intending to challenge the general structure of society. Such a definition, in its psychological and social basis, distinguishes riot in a rough way from revolutionary violence, which aims at the destruction of the existing political structure; or insurrection, the uprising of people essentially excluded from political participation; or group criminality, where people act in defiance rather than alleged support of accepted communal standards; or acts of civil disobedience, which involve lawbreaking to dramatize a cause but without threatening injury or destruction; or acts of disruption or symbolic violence such as burning in effigy where no real threat is involved. Such a definition of riot would include some types of social violence—like lynchings or vigilance committees in areas where there were existing legal structures—often given other labels. Here, aside from some very quasi-judicial procedure, the main difference from riot was the unusual inactivity of the constituted power.¹¹

Defenders of specific riots in the period talked of the action not as revolution or even illegality but as an enforcement of justice within the bonds of society—an immediate redressing of moral wrongs or a removal of social dangers that for various reasons could not be handled by ordinary legal process. Justifications of riots and vigilance committees often invoked the precedents of 1776 in their defense, but such invocations invariably implied no intention to destroy society, suggesting instead that existing society entailed the right of popular correction of social abuses in instances when the legal system was unable or unwilling to act. In the United States the “right of revolution” justified not overthrowing the government but considerable group violence within its structure.¹²

Observers realized that the traditional justifications of riot in colonial America or in Europe were absent, and they puzzled over explanations of why rioting re-established itself in the United States. In pondering the “disorganizing, anarchical spirit” of 1835, the editor of the *Boston Evening Journal* claimed, “There are strong reasons why the laws should be implic-

¹⁰ Francis Grund, *The Americans in Their Moral, Social and Political Relations* (Boston, 1837), 180.

¹¹ Some other things have been suggested to distinguish riotous violence from vigilance committees: that they were composed of respectable citizens, that they were extralegal rather than antilegal organizations, and that they did not act “spontaneously” as rioters did. In truth the composition of some riotous mobs—especially those against abolitionists or Mormons—rivaled in respectability that of vigilance organizations. Both groups saw their actions as extralegal, and both acted usually with some preparation but with much responsiveness to chance developments and moods.

¹² Eugene Dumez, introd. to Alexandre Barde, *Histoire des Comités de Vigilance aux Atakapas*, (Saint-Jean-Baptiste, La., 1861), iii, 36–37; Leonard L. Richards, *Gentlemen of Property and Standing: Anti-Abolition Mobs in Jacksonian America* (New York, 1970), 69, 97–98; Richard Maxwell Brown, “The American Vigilante Tradition,” in Graham and Gurr, *History of Violence*, 181.

itly obeyed in this country for they are but the echo of public opinion. . . . Our laws are not made as in many countries abroad, by the few for the suppression of the many, but by the many for the advantage of the whole." Governor James F. Thomas of Maryland put the case most succinctly:

In governments not formed in the principles of republicanism . . . these popular commotions may sometimes be palliated or excused. . . . But in a country like ours where the people are acknowledged to be supreme, and are in fact in the constant practical exercise of absolute sovereignty there can be no apology, there is no extenuation or excuse for such commotions, and their occurrence stains the character of the government and wounds deeply the cause of equal government.¹³

The irony in these observations was that the reason for denying the old justification of riot—the institutionally clear power of the individual to influence the state—had become central to the new. The ideological tenets and political emotions of the age of Jackson, focusing on the centrality and sovereignty of the individual, both encouraged riotous response to certain situations and made it difficult to put riot down when it broke out. Jacksonian political notions were in no sense new, but the intensity, the immediacy, indeed the simplicity with which they were held gave them a fresh cast and social significance.

American political theory had long stressed the centrality in the social structure of the individual rather than the state: here the state was to have little power, no more than was needed to safeguard, or if broadly construed, to promote the individual's pursuit of happiness and search for fulfillment. Authority was subdivided among federal, state, and local groups; it was checked and balanced on each level and bound by constitutional, natural, and democratic controls to prevent tyranny over the individual. "In contrast to Europe, where society is everything and the individual nothing, and where society crushes without pity all who stand in its way," wrote one of the ablest defenders of early vigilante groups, "in America the individual is all and society nothing. There an admirable system of laws protects the feeble, the poor, the accused; there especially is the jury favorable to the defense; and finally, there all aspects of the law are subordinated to individual right, which is the basis and essence of the republic."¹⁴ Democratic government was not only to reflect the will of the people but also was not to interfere with the proper private will of the individual.

Historians have generally seen the Jacksonian period as marking the fruition of the nation's democracy—a notion upheld even as the evidence mounts that most of the legal changes toward democracy occurred earlier

¹³ Boston *Evening Journal*, Aug. 7, 1835; Governor Thomas's message, Dec. 30, 1835, quoted in the Baltimore *Republican*, Jan. 4, 1836; see also the Baltimore *Republican*, Aug. 10, 1835; and Francis Wyse, *America, Its Realities and Resources* (London, 1846), 1: 199–200.

¹⁴ Dumez, introd., iv. Dumez concluded that the great advantages of the American system had their reverse in the difficulties of punishing wrong doers, especially among "un peuple né d'hier."

and that the major techniques of the second party system had been prefigured in the first.¹⁵ The answer may lie in seeing democracy less as a legal and technical system than as a psychological construct: Everyman's sense of his equality of right to participate and of his ability to decide. Democracy in this psychological sense reaffirms the importance of Jackson on the political scene: his lack of formal education, his intuitive strength, his belief that anyone had the ability to handle government jobs, his transformation of the presidency from that of guide for the people to a personalized representative of the Democracy, all helped create a sense of power justly residing in the hands of each man rather than in the state and a sense of the need for democratic citizens to pursue the right comparatively free from mere procedural trammels and from deference to their social and intellectual betters.

Andrew Jackson himself deplored the rioting that accelerated during his second administration. In at least three instances he sent federal troops to quell riots, and at the height of rioting in 1835 he wrote Amos Kendall, "This spirit of mob-law is becoming too common and must be checked or, ere long, it will become as great an evil as servile war, and the innocent will be much exposed." Yet in this same letter he showed his willingness to circumvent laws that he thought were protecting the guilty. He approved of Kendall's decision to let postmasters withhold abolitionist literature from the mail if they chose on the grounds that, in Kendall's words, citizens owed "an obligation to the laws but a higher one to the communities in which we live." Jackson thought Kendall had perhaps not gone far enough and suggested that postmasters be ordered to deliver abolitionist mail only at the receiver's request and that the names of all those accepting the material be published to "put them in covenant."¹⁶ Jackson's personality and actions rather than his ideas made him seem, more than any other American political figure, the anarchic hero, a man who when he decided something went ahead untouched by popular clamor in favor of the national bank, or judicial decisions supporting the Cherokees, or mere legal technicalities regarding bank deposits. The Whigs felt real fear at the implications of King Andrew's highhandedness, but their attempts to make political capital out of it foundered on popular acceptance of Jackson's own sense of his role: that he was the disinterested

¹⁵ Richard P. McCormick, "New Perspectives on Jacksonian Politics," *AHR*, 65 (1959-60): 288-301; McCormick, *The Second American Party System: Party Formation in the Jacksonian Era* (Chapel Hill, 1966), 19-31; William Nisbet Chambers and Walter Dean Burnham, eds., *The American Party Systems: Stages of Political Development* (New York, 1967).

¹⁶ Andrew Jackson to Amos Kendall, Aug. 9, 1835, Andrew Jackson Papers, Library of Congress; Amos Kendall to Alfred Huger, Charleston postmaster, Aug. 4, 1835, printed in the *Washington Globe*, Aug. 12, 1835. Richard Maxwell Brown suggests Jackson once advised settlers to punish a man by lynch law; actually Jackson said only that he had no right to pardon a man convicted by an informal jury in Iowa territory prior to the extension of United States law to the area and urged that pardon be sought from the informal authority. "Legal and Behavioral Perspectives on American Vigilantism," *Perspectives in American History*, 5 (1971): 121-25.

spokesman of the people and the Democracy and as such his actions could not abridge but only perfect democratic procedure.

Jackson fitted perfectly the popular American image of the man who need not follow accepted procedures because of the rectitude of his own character, which insured proper action in a world neatly segregated between the innocent and the guilty, the righteous and the monstrous. Jackson's popularity was rooted in his embodiment of the deepest American political myth: that man standing above the law was to be not a threat to society but its fulfillment. "Trust thyself. Every heart vibrates to that iron string," wrote Emerson in his exhortation to his countrymen to be truly self-reliant and hence to become truly representative men. And James Fenimore Cooper, intending to write a story glorifying the ways of his patrician father, created a subsidiary character who stole the novel and the affections of American and world readers. Natty Bumppo came into being as the representative of natural justice in contrast to the legal justice of the good Judge Temple, and Cooper intended to preach of the sad need for the latter to prevail. But the author himself, much less his democratic audience, had small enthusiasm for the formal dogma that Natty ought to be punished for illegally shooting a deer in a community where the respectable citizens killed maple trees for sugar and slaughtered passenger pigeons and fish for fun—legally, of course. And after Natty comes Henry David Thoreau and Huck Finn and William S. Hart and Gary Cooper and Humphrey Bogart and John Wayne and the Hemingway heroes—all men whose stature comes from their standing outside of society and the law in order to live by an individual code, which peculiarly does not threaten the social good but offers its best protection. "Self-interest rightly understood" was the term Americans used to suggest that there was no disjunction between individualism and social responsibility but rather perfect union, Alexis de Tocqueville reported—with great skepticism about how well this worked in fact.¹⁷

THE ANARCHISTIC IMPLICATIONS of these tenets of Jacksonian democracy influenced but never seriously undermined that social force which most affected the lives of citizens, the legal system. Foreign observers agreed that the United States in the 1830s and 1840s was characterized less by anarchy than by a strong conformity to accepted standards and a general adherence to laws with little external pressure.¹⁸ Yet there were paradoxes here too: in the willingness of Americans to disregard law on particular occasions with no sense of striking at society itself, their frequent

¹⁷ Ralph Waldo Emerson, "Self-Reliance," in *Complete Works* (London, 1873), 1: 19; James Fenimore Cooper, *The Pioneers* (New York, 1823), chs. 20–36; Alexis de Tocqueville, *Democracy in America*, ed. Phillips Bradley (New York, 1957), 2: 129–35.

¹⁸ Michael Chevalier, *Society, Manners and Politics in the United States: Letters on North America* (New York, 1961), 321–29; Harriet Martineau, *Society in America* (Paris, 1837), 1: 83–93; 2: 103–16; Tocqueville, *Democracy in America*, 1: 269–78; Grund, *Americans*, 155–80.

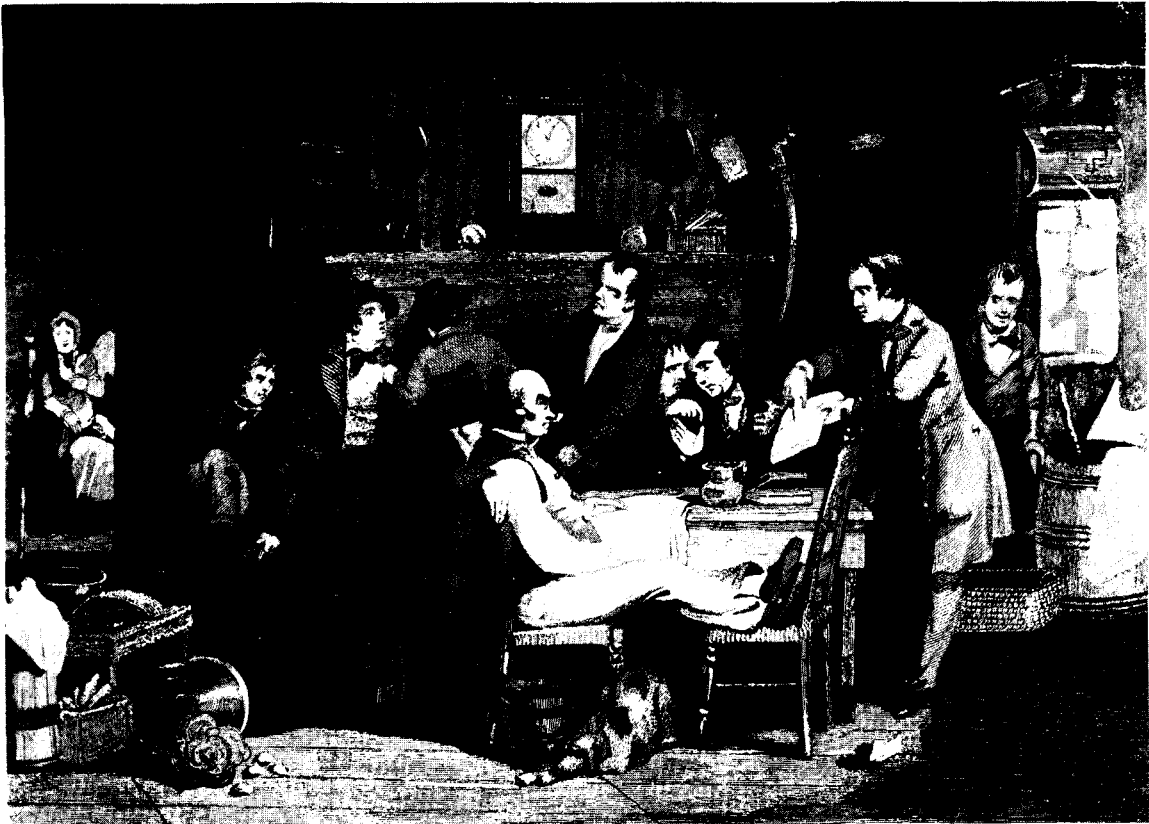


Fig. 2. A lithograph by T. E. Matteson of "The Justice's Court," an evocation of frontier legal procedures that appeared in Nathaniel Parker Willis's gift annual *The Winter Wreath* in 1853. In the accompanying description Willis wrote, "Squire Smith has a very tangled bit of jurisprudence to unravel. With his pipe lit and his legs up, he is taking it easy as far as his body is concerned, but his troubled forehead and fixed look show that his mind is very hard at work upon the merits of the case. . . . He holds court in his farmhouse kitchen, and has a neighbor who is very free with his advice. . . . Well, it is not improbable that justice will be truly administered, for Truth, though they build palaces to contain her, lodges oftenest in places more humble."

scorn of the legal process to which they had such frequent recourse, and their vehement dislike for lawyers, the "necessary evil" to which the populace nevertheless consistently gave political power.¹⁹

Richard Rush wrote in 1815 that "here law is everything," and he explained that this was so because of "an alliance between an active and restless spirit of freedom and the comfortable conditions of all classes." This con-

¹⁹ Thomas Low Nichols, *Forty Years of American Life, 1821-1861* (New York, 1937), 223-24; James Willard Hurst, *The Growth of American Law* (Boston, 1950), 3-15, 249-55, 276-85; Perry Miller, *The Life of the Mind in America from the Revolution to the Civil War* (New York, 1965), 99-116. Richard E. Ellis explores the political ramifications of antilegalism in an earlier period in *The Jeffersonian Crisis: Courts and Politics in the Young Republic* (New York, 1971). Americans' mixed feelings toward the law, although felt with a certain democratic acuteness, were of course part of man's larger ambiguous hostility to authority. The connections between this hostility and social violence are intriguingly explored in Jacob Bronowski, *The Face of Violence* (New York, 1955), and Elias Canetti, *Crowds and Power* (New York, 1963).

tentious sense of liberty and the widespread ownership and transfer of property described by Rush have generally been seen as the main impetus to law in American society. But these essentially stand as symbols for a larger truth: that as people move from a traditional society their relations must be controlled less by inherited patterns and more by formalized law. Roscoe Pound's seeing the origin of law in "codified tradition" is clearly correct, but equally important is the fact that it need be codified only when or in those areas where tradition itself is no longer strong enough to hold sway in disputes.²⁰ In the United States both abundant resources and democratic traditions allowed the benefits of the new bourgeois order to be widely shared, and it also served, as did the nation's conglomerate population, to disintegrate traditional mores quickly. Tocqueville's study of the United States revealed to him essentially how democracy tended to destroy the traditional trammels, or human bonds, of aristocracy—the historical family, the permanent community, the established church, the inherited profession and social class—both to free man and to isolate him. Man in America, except for the network of voluntary associations with which he protected himself and the mass in general with which he identified, was man alone. Because of the identification with the mass, public opinion was an effective police force ensuring general compliance with accepted standards. But in subtler and especially commercial areas of human dealing, there were neither accepted familial, communal, religious, nor traditional authorities to settle disputes. A bourgeois society, as America fairly was by 1830, must elevate law both because of what it is creating and what it has to destroy.

Americans knew they needed law and even in frontier areas tended quickly to set up legal systems and generally to respect them. The *Spirit of the Times* reported, "One of the first wants of our new settlements is a regular administration of justice, for the privilege of litigation, so far as being considered, as a witty writer has termed it, an expensive luxury, is by our free and enlightened citizens regarded as one of the prime necessities of life."²¹ Chaotic or ridiculous instances occurred in frontier law, but these were the exceptions rather than the rule and were cherished in American folklore and memory because they corroborated the illusion of freedom from oppressive technicalities.²² The heavy dependence of Americans

²⁰ Richard Rush, *American Jurisprudence* (Washington, 1815), 7–10; Roscoe Pound, *Introduction to the Philosophy of Law* (New York, 1945), 4–7. The relationship between "made" and "implicit" law is ably explored in Lon L. Fuller, *Anatomy of the Law* (New York, 1968), 43–119.

²¹ *Spirit of the Times*, 10 (Jan. 9, 1841): 543; James Willard Hurst, *The Law and Conditions of Freedom in the Nineteenth Century United States* (Madison, 1856), 3–32; Robert R. Dykstra, *The Cattle Towns* (New York, 1968), 112–48; Elizabeth Gaspar Brown, "The Bar on the Frontier: Wayne County, 1796–1836," *American Journal of Legal History*, 14 (1970): 136–56. Daniel Boorstin has argued that settlement in the United States featured community preceding legal system, but his evidence points to a different conclusion: Americans manufactured temporary legal structures whenever they ventured beyond the pale of the established system because they were not communities but simply groups of individuals held together by temporarily common objectives. *The Americans: The National Experience* (New York, 1965), 65–87.

²² Anton-Hermann Chroust, *The Rise of the Legal Profession in America* (Norman, 1965),

on law stimulated a need to remember circumventions of it because the very use of the system denied the personal independence that was the American ideal.

Covert public dislike for the legal system commonly took the form of scorn of lawyers, the intellectual elite upon whom litigious Americans depended most directly.²³ Timothy Walker's defense of the profession in his *Introduction to American Law*, long a textbook in American law schools, canvassed the common charges against lawyers. People complained of the undue complexity of the law, but Walker professionally exulted in it. "Whatever . . . may be my feelings as a man and as a citizen, as a lawyer, I am bound to rejoice in those difficulties which render our profession so arduous, so exclusive, so indispensable." Walker added "respectable" to his list of traits resulting from legal technicality, but he might better have included "profitable." The lawyer's function, Walker continued, was "to vindicate rights and redress wrongs," but in the next sentence he added, "The guilty and the innocent, the upright and the dishonest, the wronging and the wronged, the knave and the dupe, alike consult him, and with the same unreserved confidence." That guilty, dishonest, wronging knaves could place "unreserved confidence" in lawyers might for some confirm the idea that the profession delighted in chicanery and hired "out their conscience as well as their skill, to any client who will pay the fee." Of those charges, Walker told young law students, "I, for one, am willing to admit their truth to some extent. . . . We also take refuge behind the principle that supply corresponds to demand. If there were no dishonest or knavish clients there would be no dishonest or knavish lawyers. Our profession, therefore, does but adapt itself to the community." This adaptability, the moral ambiguity and sophistry, the seemingly purposeless complexity, the expensiveness charged to lawyers were all in truth a part of the legal system they represented. The attempts of legislators to make every man his own lawyer were as telling of social desires as they were futile.²⁴

Vice Unmasked, a book written in 1830 by P. W. Grayson, presents most coherently the intellectual structure of uneasiness with the law that ran through Jacksonian life. Little is known about Grayson except what he himself reveals in the book: that his criticism of lawyers is knowledgeable, he himself having been one before he repented. When the book was published, Grayson apparently had connections with the New York Work-

2: 92-128. Joseph G. Baldwin, himself a Southwest lawyer between 1836 and 1854, chronicled during these years both the amusing exceptions and the stability of general rules of frontier legal development in *The Flush Times of Alabama and Mississippi*, ed. William A. Owens (New York, 1957), 34-51, 163-82.

²³ For various kinds of popular objections to lawyers, see the *Working Man's Advocate*, Dec. 19, 1829; *Niles' Register*, 37 (Nov. 7, 1829): 169; *Cincinnati Chronicle*, Aug. 5, 1837; *North American Review*, 51 (July 1840): 234; Harriet Beecher Stowe, *Dred, A Tale of the Great Dismal Swamp* (Boston, 1856), 1: 20-21; 2: 99-110; Gerard W. Gawalt, "Sources of Anti-Lawyer Sentiment in Massachusetts, 1740-1840," *American Journal of Legal History*, 14 (1970): 283-307.

²⁴ Timothy Walker, *An Introduction to American Law, Designed as a First Book for Students* (2d ed.; Boston, 1844), 15-19.

ingmen's party; George Henry Evans published his book, and the *Working Man's Advocate* advertised it and reprinted a review of it from the *Daily Sentinel*, which judged *Vice Unmasked* an important study if "a little enthusiastic perhaps."²⁵

Grayson was unenthusiastic about law because he considered it the greatest obstacle to the realization of the promise of American life. That promise for Grayson, as for many Americans, had been to free man's potential by lifting from him the weight of the superstitions and repressive institutions of the past. The American government was the beginning of improvement, but progress was still slight, as the injustices and inequalities and unhappiness of America amply showed. What had gone wrong? Grayson's answer was simple: the United States had ended repressive government but had left untouched a legal system that impeded man's freedom and hence tarnished his natural integrity. Grayson fervently summarized the complaints against lawyers: they were a class of men who had a vested interest in fomenting and prolonging disputes; who were essentially social prostitutes willing to take any position that the highest bidder for their talents desired; who eschewed any concern for pursuing truth in order to pursue their client's interest; and who exulted in the complexities of their profession because these prevented honest men from acting in their own interests. Yet Grayson's prime target was not lawyers but the system that encouraged their moral degradation. The law itself was a jumble of old formulas inherited from feudal times, rarely suited to modern instances, and always more helpful in telling the cunning man how much he could get away with than in setting positive standards of human conduct. And whatever was done to improve it only changed its façade; one passed bankruptcy laws to protect the poor debtor, but the speculative stockjobber made use of them to defraud his honest creditors. Weaving together Thomas Paine's and a transcendental vision of man's potential, Grayson centered his indictment on the effects of law on the "moral essence of man," the way it debased man's sense of self and social responsibility by turning him from his high moral potential to a tricky tailoring of conduct to avoid legal prosecution. In short, law was generally a tool of the cleverly vicious, a snare for the simply virtuous, and a burden on everyone, crippling human decency and progress.

Practically, Americans were not about to accept Grayson's program that law should deal only with instances of gross physical attack and in all other areas let man "seek, by the light of his own conscience, in the joyous genial climate of his own free spirit, for all the rules of his conduct." But Grayson's thinking paralleled that of many other Jacksonians.²⁶ Indeed his ma-

²⁵ P. W. Grayson, *Vice Unmasked, An Essay: Being a Consideration of the Influence of Law on the Moral Essence of Man* (New York, 1830); *Working Man's Advocate*, Mar. 6, 1830. The paper reported that Grayson had at one time been a member of the Kentucky state legislature from Louisville.

²⁶ Grayson, *Vice Unmasked*, 168. For other pleas for near-legal anarchism see the speech

jor premise was perfectly correct: from a tough-minded point of view, law, as Oliver Wendell Holmes, Jr. argued later on, has much less to do with man's highest responsibilities than it does with telling bad men just how much they can get away with; in any legal system decisions must be based as much on technical requirements as on the unfettered pursuit of justice. "Law and right," wrote John Quincy Adams, "we know but too well by the experience of mankind, in all ages, including our own, are not convertible terms."²⁷ Such was the paradox of legal development: the desire to be free from individual power and whim impelled rational man to set up a judicial structure that inevitably impeded almost as much as it promoted perfect justice. And so mankind's favorite myths of justice once again enthroned personal wisdom: the judgment of Solomon, Louis IX under the oaks, Cervantes' Sancho Panza, and any number of wise men ensuring the triumph of right in fairy tales or melodramas.²⁸

Both popular animus to the law and its importance in the lives of citizens helped to make the Jacksonian period, in Pound's phrase, "the formative era" of American law. The militant majoritarianism of Jacksonian rhetoric also spurred the efforts of conservatives to strengthen what seemed to be the only brake on untrammelled popular will. Francis Bowen wrote, "Here, nothing stands between the individual citizen and *his* sovereign—the majority of the people,—but the majesty of the law and the independence of the courts." Conservatives like Joseph Story, Lemuel Shaw, Timothy Walker, and a host of lesser figures were so successful in increasing legal learning, dignity, and responsiveness to social needs that legal antipathy was never able to become programmatic. Even the codification issue, which drew on many of Grayson's ideas in "practical" form, was neutralized by conservative judicial skill and flexibility.²⁹ Such efforts ensured that Jacksonian America became increasingly a government of laws not men, but democratic man was not entirely happy about it.

of 1837 by John M. Hunt quoted in Fitzwilliam Byrdsall, *The History of the Loco Foco or Equal Rights Party* (New York, 1842), 149–50; and the *Democratic Review*, 6 (Dec. 1839): 466–72 and 18 (Jan. 1846): 26–30.

²⁷ Oliver Wendell Holmes, Jr., *Collected Legal Papers* (New York, 1952), 169–74; John Quincy Adams, "On the Opium War" (1841), *Massachusetts Historical Society Proceedings*, 43 (1910): 304. Roscoe Pound describes the inevitable judicial tension between doing justice in the immediate case and establishing desirable precedents for future ones. *The Formative Era of American Law* (New York, 1938), 119–24.

²⁸ The plays of the early nineteenth century, fairly accurate reflectors of popular ideals, often used the motif of justice ensured by personal wisdom. The first popular melodrama in the United States, William Dunlap's translation of Louis Charles Caigniez's *The Voice of Nature* in 1803, was an updating of the Solomon and true motherhood story; and Royall Tyler, at one time chief justice of the Vermont Supreme Court, wrote plays glorifying the judicial wisdom of both Solomon and Sancho Panza. *Four Plays*, ed. Arthur W. Peach and George F. Newbrough (Princeton, 1941). The most successful American comedy of the 1840s showed a simple farmer distributing justice in the end to avoid mere legal solution for criminal acts. Anna Cora Mowatt, *Fashion; or Life in New York* (London, 1850), act 5.

²⁹ Francis Bowen, "The Independence of the Judiciary," *North American Review*, 57 (Oct. 1843): 420; William W. Story, *Life and Letters of Joseph Story* (Boston, 1851), 1: 448; 2: 241–51, 570–600; Leonard W. Levy, *The Law of the Commonwealth and Chief Justice Shaw: The Evolution of American Law* (New York, 1967), 303–36.

Within the legal structure, popular wariness about law appeared in the leniency with which juries tended to view offenders for whose crimes there were extenuating circumstances.³⁰ Such legal actions, if technically irregular, often provided a kind of rough equity that the law could not formally incorporate. For instance, a St. Louis jury acquitted an actress who had stabbed her faithless lover to death in the theater one night. The argument that the man had a bad heart that might have given way before the knife got there was something of a blow to technical justice, but who knows if eternal justice would have been better served by a conviction? And one sympathizes with the California judge who concluded his charge to a jury: "Well, gentlemen, that's the law, but I don't really think it's God Almighty's justice, and I guess you may just as well find for the defendant."³¹ Democratic man admitted that a legal system was needed, but he had an active responsibility to see that it did not contradict the will of God Almighty, as interpreted by the individual of course.

THIS REFUSAL TO ACCEPT the sanctity of the law had its most disruptive manifestations in the long series of riots in the Jacksonian period. These were very diverse in origin and goal, but patterns do emerge about the structure of rioting, its social and psychological results, the type of person who rioted, and the problems of riot control in the period. A consideration of two incidents and of an ethnic category of riot suggests something of the nature of social violence in Jacksonian America and its implications for riot theory in general. The Bank Riot in Baltimore and the Snow Riot in Washington, D.C., took place within a week of each other in early August 1835. The Baltimore riot was highly unusual in that the mob attacked their social and economic superiors; partly for this reason information on it is unusually abundant. The Washington riot is more typical both in its direction and in the sketchiness of the material available on it. Together the two constitute an introduction to Jacksonian social violence.

Both riots took place in a climate of national near-hysteria. Instances of riot and lynching around the country filled the Baltimore and Washington newspapers throughout July and August. The same day that major rioting broke out in Baltimore, its leading periodical, *Niles' Register*, began with "a great mass of curious and important matter" showing that "the state of society is awful. Brute force has superseded the law, at many places, and violence become the 'order of the day.' The time predicted seems rapidly

³⁰ Dumez, introd., iii-vi. Francis Bowen chided James Fenimore Cooper for his novelistic attack on the jury system, *The Ways of the Hour*, on the grounds that the central argument of the book was mistaken: American juries did not tend to convict unfairly but were notorious for their leniency even when evidence of guilt was substantial. "Cooper's *Ways of the Hour: The Trial by Jury*," *North American Review*, 71 (July 1850): 121-35.

³¹ Noah M. Ludlow, *Dramatic Life as I Found It* (St. Louis, 1880), 550; Sol Smith, *Theatrical Management in the South and West for Thirty Years* (New York, 1868), 165, 192; George Templeton Strong, *Diary*, ed. Allan Nevins and Milton Halsey Thomas (New York, 1952), 2: 81.

approaching when the mob shall rule." Niles blamed both lawless mobs and "fanatics who . . . have set their presses at work to spread desolation and death through the whole south" for the trouble.³² The most obvious precipitant of these "various excitements" was the growing effectiveness of abolition organization in the North and the sending of abolitionist literature southward. The South responded with fear and fury, claiming that these movements would create slave insurrection. The South threatened economic boycott against the North, and Northern commercial centers responded with huge public meetings condemning abolitionism and declaring that the North had no proper business even discussing slavery. Such declarations of sentiment failed to satisfy the South, which demanded "works" not "words"—specifically laws prohibiting the discussion of slavery and legal or illegal action to silence those promoting abolition. Laws curtailing freedom of speech or mobs seemed the only possible response to appease extreme Southern feeling.³³

The political situation of the nation further heated these passions. Martin Van Buren, running for the presidency as Jackson's chosen successor, was politically vulnerable in the South, while the leading opposition candidate, Jacksonian renegade Hugh Lawson White of Tennessee, had great strength there among both Jacksonians and Whigs. Since the South



Fig. 3. Southern attacks on abolitionists in 1835 pointed to this woodcut, illustrating an antislavery poem by John Greenleaf Whittier, as primary proof of the insurrectionary intent of abolitionist literature. Why else, they asked, show a picture of a slave in chains with such a motto? The motto and woodcut were an adaptation of the seal of England's Committee for the Abolition of the Slave Trade, which had become the pictorial symbol of the British antislavery movement. Illustration from the Library of Congress.

³² *Niles' Register*, 48 (Aug. 8, 1835): 397. A survey of shocked reaction to mobs from newspapers around the country appears in the *National Intelligencer*, Aug. 14, 1835.

³³ *Niles' Register* between July and November gives a vivid picture of this controversy, complete with the proceedings of the various meetings and a rich selection of editorial comment from all sections and parties. Until mid-September sentiment was almost wholly antiabolitionist, but this changed as the implications of Southern demands became clear and the Northern nonparty press became truculent against Southern extremism. Niles himself marks the shift. On September 19 he wrote that Southern willingness "to have mob-law—or accept of regulations that monarchs would 'turn pale' to think of" was a remedy considerably "worse than the disease" of abolitionism. 49 (Sept. 19, 1835): 33.

promised to be the central battleground for supremacy, the Whig and Democrat partisan press competed in rabid attacks upon the abolitionists. In many instances the press and leading politicians promoted proslavery mob action;³⁴ more commonly they condoned them with open expressions of approval or quiet toleration. No Baltimore or Washington newspaper directly encouraged these particular riots, but even nonpartisan papers edited by men deeply disturbed by riot tended to print without comment incidents of proslavery violence or to deplore them while laying major blame on "those unprincipled incendiaries," the abolitionists who were their victims.³⁵ Well over half of the riots in July and August of 1835 had no immediate connection with abolition, but they all sprang from a social climate with an extraordinary tolerance for riot. Circumstances allowed only a very few newspapers or public figures to take a strong stand against the best known of popular outbursts.

The Baltimore riot was an attack on those connected with the failure to settle the affairs of the Bank of Maryland, which had ceased operation almost a year and a half earlier. "Considerable numbers of people, 'good, bad, and indifferent' " congregated in Monument Square during the clear and pleasantly cool evenings between August 5 and 7.³⁶ The general topic of conversation was the action of the trustees and the "secret partners" of the bank, and the mood was one of frustration and anger at the prolonged legal obstacles to settlement. On Friday afternoon Mayor Jesse Hunt called a public meeting, which pledged itself to keep the peace and to try to discover the source of the inflammatory handbills posted on walls, but which also requested that the bank's books be immediately opened for public investigation. A crowd of ten thousand gathered on Friday night, but, aside from minor rock throwing, the peace was kept. On Saturday the trustees

³⁴ The clearest case of politically instigated riot was the antiabolitionist mob in Utica in late October 1835. Van Buren, embarrassed by the comparatively mild New York City resolutions damning abolitionists, had his friends organize an Albany meeting, which passed stronger pro-Southern resolutions than those of any other city. The administration Richmond *Enquirer* praised them as "free from all qualification and equivocation—no idle denunciations of the evils of slavery—no pompous assertions of the right of discussion," but the Richmond *Whig* pointed out that they wanted only "the recognition of the power of the legislature to suppress the fanatics, and the recommendation to do so," and without this they were meaningless. Niles reported much Southern pressure on Van Buren to add legislative proposals to the resolves, and omission of these proposals was, for Niles, conclusive proof that nothing legally would be done. *Niles' Register*, 49 (Oct. 3, 1835): 75. About three weeks later, Samuel Beardsley, a Jacksonian congressman, led a mob that drove the New York Anti-Slavery Society out of Utica and sacked a press that embarrassed the Democratic party by being both antislavery and pro-Van Buren. Administration papers immediately touted this as proof of the purity of Van Buren's Southern feelings, and hinted that Beardsley would be appointed governor of New York if Van Buren were elected and could find a place in Washington for Governor William Marcy. Marcy stayed in New York after Van Buren's victory, but quickly appointed Beardsley the state's attorney-general. Another Democratic governor appointed him to the New York Supreme Court, and he became chief justice briefly in 1845.

³⁵ Georgetown *Metropolitan*, Aug. 12, 1835; Washington *Mirror*, Aug. 1, 1835; Baltimore *Gazette*, Aug. 3, 1835; *Niles' Register*, 49 (Oct. 31, 1835): 149.

³⁶ William Bartlett to Edward Stabler, Aug. 12, 1835, printed in *Maryland Historical Magazine*, 9 (1914): 157. The weather reports are in Henry Thompson's diary, in the Maryland Historical Society, Baltimore.

announced that the bank's books were impounded by the Harford County Court, and the mayor organized a citizens' guard armed with two-foot long poplar sticks. That evening the guard managed to keep the mob from their main objective, the home of the leading "partner," Reverdy Johnson, but were unable to prevent the sacking of the house of another "partner." When the crowd bombarded the guard with stones and brickbats and wounded several guards seriously, some of the guard's leaders demanded the right to use guns, and the mayor reluctantly consented. At least five people were killed, and some ten or twenty were wounded by shooting. Sunday morning the mayor announced that the firing had been done "against my will and advice," and the leaders of the citizens' guard decided that prudence dictated leaving town. The mob was left unopposed. That evening and night hundreds systematically sacked and damaged the homes of the mayor and of four men connected with the bank and did minor damage to property of certain leaders of the citizens' guard, while thousands watched.³⁷ At about noon Monday, eighty-three-year-old Samuel Smith drove through the streets in a carriage flying an American flag to a large meeting where he effectively organized the citizens into an armed force to handle any further troubles. The destruction, which was still going on, immediately ceased, and the citizen patrols that guarded the city for well over a week met no opposition.³⁸

Federal troops were dispatched to Fort McHenry near Baltimore shortly after Smith and his fellow citizens had the situation under control, but most troops were quickly withdrawn to Washington, D.C., to overawe a riot that flourished between August 12 and 14 and sputtered on for several days thereafter. This riot resulted primarily in an attack on the property of free blacks. The mob congregated on Tuesday when it became known that abolitionist literature had been found in a trunk owned by Reuben Crandall, who was staying in Georgetown and was the brother of Prudence Crandall, already well known as a victim of riot when she had attempted to teach black girls in her school in Canterbury, Connecticut.³⁹ Crandall was quickly arrested and arraigned in jail to prevent his falling into the hands of the mob, who then turned their attention to a restaurant, the Epicurean House, which was run by a mulatto, Beverly Snow. Before the mob arrived, Snow had wisely disappeared; after a fruitless search for anti-slavery writings, and some more successful drinking, the mob left. The next day they returned to "get Snow," but again he escaped. A search of the

³⁷ This account is constructed from newspaper descriptions in the *Baltimore American, Republican*, and *Gazette*, from accounts in other papers reprinted from the *Baltimore Chronicle* and *Patriot*, and the testimony taken by the Maryland General Assembly on the riots, published in 1836. There are no major discrepancies about the facts of the incident and no probing considerations of its structure.

³⁸ When James Gordon returned to Baltimore a week after the riot, he reported the "military arrangement of cannon and soldiers" looked "more warlike" than anything he had ever seen. *Diary*, Aug. 17, 1835, Maryland Historical Society.

³⁹ *The Trial of Reuben Crandall Charged with Publishing and Circulating Seditious and Incendiary Papers* (Washington, 1836).

homes of other free Negroes resulted in finding some abolition newspapers in the house of James Hutton, who was hustled to jail to protect him from the mob. The crowd, after staging a public meeting the proceedings of which no newspaper reported, returned to Snow's to destroy his property, "not forgetting to crack a bottle of hock, 'now and then.'" That night, and intermittently during the next week, the crowd burned or stoned several black-owned buildings.⁴⁰

SUCH IS THE SKELETAL HISTORY of the two events. A closer examination of these two riots shows much not only about their own structure but about the nature of rioting in the Jacksonian period. The central question, of course, concerns what motivates riot. Seventy-five years ago Gustave Le Bon began the sociological study of crowd behavior with a discussion that can hardly be taken seriously today but that still informs much thinking about the problem. Man acting singly, Le Bon argued, acts rationally, but acting in groups—he lumps together such things as parliamentary bodies and riotous mobs—they revert to instinctual behavior; "bestial," "primitive," "childlike," "feminine" were Le Bon's favorite adjectives for it. Le Bon's explanatory devices are largely funny; the most irrational of crowds, Latin ones, he tells us, are so because they are "the most feminine of all." Yet his ideas have remained provocative because behavior within mobs suggests disruption more than continuity in the character of the participants, and aspects of his description are still convincing, particularly his emphasis on the emotive volatility, psychological release and anonymity, and the sense of total and totally justified power that comes from being part of a riotous crowd.⁴¹

Sociologists have long raised questions about Le Bon's arguments, especially his emphasis on the irrationality of crowd behavior.⁴² Historians, particularly Eric Hobsbawm and George Rudé, first directly attacked the irrationality hypothesis in regard to riotous violence and suggested that rioters tend to act not irrationally but in ways made understandable by their social situation and related integrally to their social needs and desires.⁴³

⁴⁰ The Washington papers all reported the riot, but were very reticent about the destruction, particularly in the month following its supposed settlement. They agreed in what they reported. Duff Green's *U.S. Telegraph*, August 13, and Francis Blair's *Globe*, August 19, expressed some regret that Crandall was not hung, but the other papers—the *National Intelligencer*, *Washington Sun*, *Mirror*, *Georgetown Metropolitan*, and *Alexandria Gazette*—regretted the rioting without condemning the rioters.

⁴¹ Gustave Le Bon, *The Crowd: A Study of the Popular Mind*, ed. Robert K. Merton (New York, 1960), 35–59. A pervasive problem in the book is that Le Bon treats three groups as identical—mobs, for which his description is most valuable; a mass society, about which his comments are inferior to Tocqueville's in complexity and suggestiveness; and decision-making bodies such as juries and parliaments, for which his analysis is at best quaint.

⁴² A good survey of the sociological theories of crowd behavior is Stanley Milgram and Hans Toch, "Collective Behavior: Crowds and Social Movements," in Gardner Lindzey and Elliot Aronson, eds., *Handbook of Social Psychology* (2d ed.; New York, 1968), 4: 542–84.

⁴³ Eric J. Hobsbawm, "The Machine Breakers," *Past and Present*, no. 1 (1952): 57–70; Hobsbawm, *Primitive Rebels: Studies in Archaic Forms of Social Movements in the Nineteenth*

Certainly Rudé and Hobsbawm win the argument if rationality means simply an understandable response to a social situation that made people discontented. Such a criterion, however, excludes irrationality by definition, for no action can be wholly unrelated to man's unhappiness stemming from objective social experience; the lunatic who thinks he is Napoleon obviously does so because this illusion fulfills certain needs caused by real deprivations and traumas in his social history.⁴⁴

Jacksonian riots do not readily fall into categories of either "irrational" or "socially purposive" behavior. The mobs in Baltimore and Washington were not particularly wanton or vicious. In both cases the action taken was sensibly directed toward the social source of riotous anger—the financial manipulations of some rich men in one case and the pretensions to social dignity of blacks in the other. The Baltimore mob was particularly fastidious. They refused to sack houses of intended victims when they were informed that they were still officially owned by the contractor or were the property of the would-be victim's mother; they put out fires in houses that they were busy demolishing so adjoining property would not be endangered; they reprimanded some people for stealing rather than destroying property; and they voted by a slim majority not to burn a lumber yard because it threatened an adjacent one. (The minority had argued that it could be safely burned if the fire trucks were called out to keep the flames in bounds.) Their chief victims were rationally chosen: four men—Reverdy Johnson, John Glenn, Evan T. Ellicott, and Hugh McElderry—who had been "secret partners" in the Bank of Maryland and who had avoided settlement of the bank's affairs through legal prosecution of its former president and his relatives; John B. Morris, one of the trustees of the bank since its failure, who had steadily supported the nonsettlement policy; and Mayor Hunt, who was blamed for the firing into the crowd on Saturday. None was guilty of indictable offenses, and Hunt and Morris had not obviously profited from the situation, although the latter had lent himself to the delay and to the publication of a very inaccurate report of the bank's situation under the influence of his legal counsel, Reverdy Johnson. This unconscionable postponement worked a great hardship on the unusual number of people of modest means—"widows and orphans, small dealers and thrifty persons,

and Twentieth Centuries (Manchester, 1959); George Rudé, *The Crowd in the French Revolution* (Oxford, 1959); Rudé, *The Crowd in History: A Study of Popular Disturbances in France and England, 1730-1848* (New York, 1964); Hobsbawm and Rudé, *Captain Swing* (New York, 1968). Rudé in his two books both defends his crowds' general social purposiveness and also argues that the crowds that followed—those after the French Revolution in the first book (pp. 209, 238-39) and in the "industrial" age that had fully arrived by 1848 in the second (pp. 218-34, 266-68)—were purged of certain backward qualities through the development of "a stable social-ideological content" (p. 234). The opposite may be the case: as popular movements developed ideological content and as Western governments opened channels of influence to a broader segment of their population, significant social protest took less violent and destructive forms, and riotous mobs became commonly more backward looking or self-indulgent.

⁴⁴ R. D. Laing, in his *Politics of Experience* (New York, 1967), suggests that insanity is often reasonable response to social inconsistencies in an existential reworking of the Romantic preference for madness to bourgeois normality.

mechanics and others”—who were the bank’s creditors, while its debtors, including the partners, profiteered shamelessly.⁴⁵ Given the complexity of the affairs of the Bank of Maryland, and judging by the mob’s choice of victims, the Baltimore mob was not only rational but financially astute.⁴⁶ Less is known about the specific mob actions in Washington, but there is evidence of similar restraint and selectivity. When told that the building and many of the furnishings of Snow’s restaurant were actually owned by others, the mob confined itself to destroying the sign and a few things of minor value. And the property they attacked—black businesses, schools, churches, and homes—were those things most contributive to the free Negroes’ sense of status and dignity, however tenuous, in the community.⁴⁷

Yet are selectivity and aspects of moderation incompatible with the idea that a riotous crowd unleashes elements of emotion that in important ways distort reality and allow individuals to act in a manner at variance with their usual behavior? The ablest defender of the Baltimore mob stressed its restraint and the justice of its social position: “fraud produced violence,” and the people “operated upon the republican maxim ‘resistance to tyrants is obedience to God.’” But he also stressed the emotive quality of the crowd situation. When the mob in Monument Square became active “every countenance was flushed with the spirit of destruction—reason had thrown down the reins and ungovernable fury had taken them up,” and the retreat of the city guard led to Sunday’s “anarchical desolation and mournful paralysis of reason.”⁴⁸ The handbills, which were sent through the mails and posted on the walls of Baltimore during the week preceding the riot, reveal this tying of highly irrational emotionalism to very real grievances:

Arm! Arm! . . . —my Countrymen—Citizens of this Republic, and of this City, will you suffer your firesides to be molested—will you suffer your beds to be polluted—will you suffer your pockets to be rifled and your wives and children beggared. . . . Then arouse, and rally around the free and unbiass’d judge Lynch who will be placed upon the seat of justice and the people enmasse will be the members of the Bar, and these lions of the law shall be made to know that the

⁴⁵ *Niles’ Register*, 46 (Mar. 29, 1834): 65. The bank had attracted small investors by offering interest on short term deposits. A letter of a retired sea captain, Thomas Williams, gives a vivid sense of the suffering the bank’s prolonged trusteeship caused its unfortunate creditors. *Baltimore Republican*, Mar. 19, 1836.

⁴⁶ The conflicting accounts of the affairs of the Bank of Maryland were presented in two pamphlets by its former president, Evan Poultney, and in two replies by Reverdy Johnson and John Glenn, the second reply appearing a few days prior to the riot. Poultney argued that he and five other men had secretly controlled the bank, that there were sufficient funds to meet, or almost meet, all debts, and that the legal cases against him and his brothers were all ruses. Poultney’s story was borne out by the bank’s books, the results of the criminal cases, and the final settlement of the trust over four years after the bank’s failure, when the full amount of debts was paid plus a ten cent dividend per dollar. This almost all went to speculators who had bought the credits at about a quarter of their value. The best accounts of these financial manipulations are a long letter from George Gibbs to the Union Bank of Tennessee, July 1834, in the Jonathan Meredith Papers, Library of Congress, and Thomas Ellicott’s self-serving but generally accurate, *The Bank of Maryland Conspiracy* (Philadelphia, 1839).

⁴⁷ The Washington papers talked most of the burning of a house of prostitution to suggest the riot had a moral tone; presumably it was owned or run by blacks.

⁴⁸ “Junius” to the *Baltimore Republican* Jan. 4, Feb. 26, Mar. 5, 31, 1836.



Fig. 4. This cartoon, published in Baltimore in 1834, reflected popular anger against the "secret partners" of the Bank of Maryland. Reverdy Johnson and John Glenn go the whole hog—the cart is drawn by "Entire Swine"—in taking money from the bank to their General Insurance Company. The money is labeled with the initials of the partners: Johnson, Glenn, Evan T. Ellicott, Hugh McElderry, and David M. Perine. Perine turns back widows and orphans under a playbill advertising "Honest Thieves" and "The Warlock of the Glen." In the background the sign Savings Institution has been changed to Losing Institution, and monkeys, symbols of avarice (see Lester K. Little, "Pride Goes before Avarice: Social Change and the Vices in Latin Christendom," *AHR*, 76 [1971]: 35, 37–38, 44), dangle money in front of respectable citizens who piously intone "Judge not that ye be not judged." Illustration from the Maryland Historical Society.

people will rise in their majesty and redress their own grievances. . . . Have not the whole Bar and the judges linked in a combination together, and brow-beaten these very people out of their just rights, with a full determination to swindle and rob the industrious and poor part of the community out of their hard earnings. . . . Designing lawyers and lazy greedy speculators . . . , these smiling villains nearly all of them are building palaces and riding in their carriages with the very money taken from the poor laborer, orphans and honest hard working mechanics. . . . Want staring your poor heart broken wives in the face—your little children clinging around their mother, crying mother, mother a piece of bread—I say mother bread—O! mother give me some bread,—while these protected villains are roling in luxury and ease, laughing to scorn the people they have just robbed. These very villains stroll the streets with a bold and impudent assur-

ance and pass for honest men—not satisfied with robbing you of your money, but treat you as Vassals to their noble lordships—to gratify their Venery desires hire pimps and procuresses to go polute your wives and prostitute your daughters—Gracious God!—is this our fair famed Baltimore—is our moral city come to this. . . . We have a remedy, my fellow citizens—Judge Lynch will be notified that he is at our head, and will take his place upon the bench—his maxims are Virtue, honesty, and good decent behavior—his remedies are simple, Tar and Feathers, effigys, gallowses and extermination from our much injured city—the victims that fall under this new law, I hope will be Johnson, Morris, Glenn, McElderry, Freeman and that dirty fellow Bossier etc., etc., etc.—

Let the warhoop be given . . . Liberty, Equality, Justice or Death!!!⁴⁹

Here a generalized social fury melted justified anger and real economic hardship into an amalgam of major democratic grievances and fears: resentment at the deviousness and elitism of the law; a hatred of the powerful, the pretentious, the learned, the rich; uncertainties about economic status and the moral stability of the family. All these fears could be welded together and expressed because they resulted not from any intrinsic flaws in society but from the machinations of specific villains—in this case five men and three etceteras. When Judge Lynch had those people “exterminated from the city,” supposedly Baltimore and the United States could return to their “fair fame” and purity.

The talk in the circular of “Venery desires” shows how the anger of the mob also united wholly separate incidents. The inclusion of “Bossier” in the list of intended victims makes clear that the author joined a recent Baltimore scandal with the long-brewing Bank of Maryland controversy. Over a week before the riot, Joseph Bossière was assaulted by an irate guardian who found his ward in Bossière’s house. Rumor had it that Bossière had seduced her and that the directress of the exclusive school where the girl boarded had acted as procuress. The moralistic anger over this incident, totally unconnected with the bank controversy, nonetheless merged with it in the minds of the rioters. The mob attacked the house where Bossière was staying, and to save it he gave himself up to the crowd; what was done to him was not reported.⁵⁰

In the Washington riot a similar event that had occurred about a week

⁴⁹ Handbill, in vertical file, Maryland Historical Society. Other handbills are in the David M. Perine Papers, Maryland Historical Society, and one is copied in William Bartlett’s letter in the *Maryland Historical Magazine*, 9 (1914): 161–62. The “Freeman” mentioned in the circular was W. H. Freeman, another Baltimore banker, to whom the Union Bank of Tennessee sold its credits of some \$275,000 on the Bank of Maryland for \$60,000. Freeman owed the bank \$50,000, so for an additional \$10,000 he bought over \$200,000 worth of credits. The mob leaders must have learned of the transaction even before the *Baltimore Gazette* announced on August 8 that the claims of the Union Bank of Tennessee had been “satisfactorily adjusted.” John Bass to Jonathan Meredith, July 30, 1835; A. Van Wyck to Jonathan Meredith, Nov. 6, 1835, Meredith Papers; “One of the Mob” to Brantz Mayer, Aug. 12, 1835, Brantz Mayer Papers, Maryland Historical Society.

⁵⁰ Bossière’s statement about his treatment during the riot appeared in the *Baltimore Gazette*, Aug. 21, 1835, and in the *Baltimore American*, Sept. 16, 1835. Reports of extreme excitement over the alleged seduction appeared in the *Baltimore Republican*, July 24, 1835, and in the *Georgetown Metropolitan*, Aug. 1, 1835.

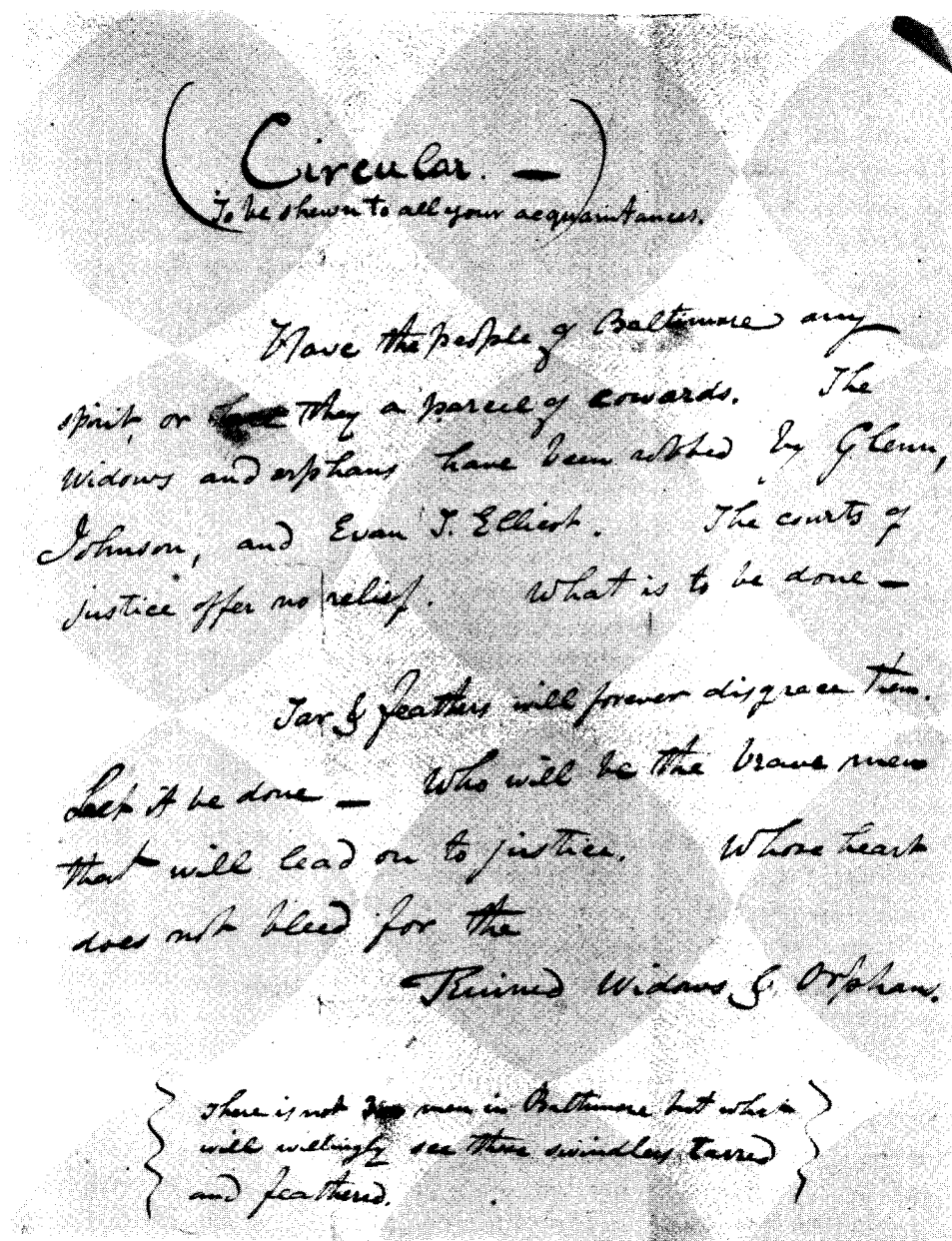


Fig. 5. Handbills such as this circulated throughout Baltimore in the first week of August 1835. Sent through the mails to tavern and oysterhouse keepers, the bills urged the recipients to circulate them and post them on walls. This handbill and several others, similar in content but different in phrasing, are in the David M. Perine collection, Maryland Historical Society.

prior to the disturbance influenced the emotion of the mob. The slave of a prominent Washington widow entered her bedroom at night with an ax and drunkenly threatened to kill her. Newspapers soon had him spouting "abolitionist jargon" as he made the attack, and the conservative *National Intelligencer* labeled the story "The First Fruits." While the widow, convinced that the slave had simply been drunk, hid him in her home and tried to sell him to safety, the press flaunted the incident as proof of coming terrors if abolitionists were not muzzled.⁵¹ Hence Crandall's supposed activities could be seen as part of a plot threatening widows with violent death, and the attack on Negroes could then go forth in the guise of saving society from servile war. The purity of the family motif was also strangely tied to the attack on Snow. When no abolitionist literature was found in his restaurant, the official charge against him became that he had insulted the honor of mechanics' wives and daughters.⁵²

In addition to a triggering generalized moral fury, another emotional set characterized Jacksonian riots. Once action began, anger was replaced by joy and release if the mob was not seriously opposed. The few reports of the Washington mob suggest great good humor, almost Bacchanalia, as the crowd destroyed, partly by drinking, the contents of Snow's Epicurean House. The reports of the Baltimore riot trials give a vivid sense of their saturnalian quality. Several rioters were convicted largely because they had lustily bragged about their riot exploits. James Spencer, furious against Mayor Hunt because Spencer had had his "knuckles shot off" on Saturday, amused the crowd as he broke Hunt's dinnerware on the street: "Gentlemen, who wants to go to a tea party, but stop I'll go and get the plates." Particular care and delight was taken in burning Reverdy Johnson's law library. The mob emptied Johnson's and Glenn's wine cellars and referred to the wine they abundantly drank as "American blood," perhaps suggesting that it was squeezed from their townsmen's labors as well as evoking older rituals of saturnalia, in which the continuance of patterns of authority was made acceptable by their brief ritual cessation. One rioter on Monday, before order was restored, went around saying "damned if he wasn't Mayor of the city" and appointing various friends to official positions. Such precise parallels to saturnalia's mock king were doubtless rare, but the mood often suggested the joy that comes from the destruction of official authority and its brief bestowal on self. Fifes and drums played and crowds of thousands watched the destruction, laughing and cheering on the rioters.⁵³ The moral

⁵¹ Mrs. William Thornton, diary, Aug. 5-8, 1835, Library of Congress; *National Intelligencer*, Aug. 8, 1835. The slave was sentenced to death, but Mrs. Thornton's ceaseless lobbying for him among her influential friends gained him a pardon from Jackson. See her diary, July 6-7, 1836.

⁵² Snow begged for a hearing from a jail in Fredericksburg where he had gone for protective custody; the *National Intelligencer* published it, but apologized abjectly when a "respectable citizen" wrote a diatribe against the paper's so honoring one of Snow's "insolent class." "Let all blacks become subordinates and laborers," concluded the citizen, or leave Washington. Aug. 27, 28, 1835.

⁵³ Hester Wilkins to her sister Mrs. John Glenn, Aug. 11, 1835, John Glenn Papers, Maryland

or social issues that gave mobs life always circumscribed their action, but such restraints coexisted with a high degree of emotive fury and joy in power that transfigured social reality. Total self-righteousness, well or ill founded, joined with the unity and anonymity of the crowd to allow a saturnalia where social man's usual restraints could be shucked.

A bank clerk, witnessing a riot in 1843, was surprised that so little was done to protect the black victims:

But the mob of Cincinnati must have their annual festival—their Carnival, just as at stated periods, the ancient Romans enjoyed the Saturnalia, and our city dignitaries must run no risk of forfeiting their “sweet voices” at the next charter election by any unceremonious interference with their “gentle violence”—their practical demonstrations of sovereignty.⁵⁴

The cross-examination of a Baltimore defense witness is telling. Asked if he had been in Morris's house, the reporter recorded the witness as saying, “Not sure—thinks he went in—don't know if he went upstairs, if he did he might have been insane—drank two or three glasses from a decanter—was ‘pretty warm.’” Had he been at Hunt's? “Might have been in the house, didn't know—thinks he was sober—was a ‘little warm,’ might have been insane—a great many passions make a man insane beside liquor—excited to see so much property destroyed.”⁵⁵

MOST JACKSONIAN RIOTERS were neither the “dregs of society,” as Reverdy Johnson called the Baltimore mob, nor so much of a social elite as Richard M. Brown and Leonard Richards have found composing vigilante or anti-abolition mobs.⁵⁶ Of the twelve people convicted of riot in Baltimore, eight can be identified as to profession: three carpenters, two pavers, one blacksmith, one hatter, and one laborer. Of the ten people acquitted, four clearly

Historical Society. The quotations are all taken from the riot trials in the Baltimore City Court, published in the *Baltimore Gazette* between late November 1835 and the end of January 1836.

⁵⁴ James W. Taylor, “A Choice Nook of Memory”: *The Diary of a Cincinnati Law Clerk, 1840–42*, ed. James Taylor Dunn (Columbus, 1950), 40. Ralph W. Conant suggests the idea of riot as saturnalia, but treats it as a largely benign phenomena. “Rioting, Insurrection and Civil Disobedience,” *American Scholar*, 37 (1968): 425–26. Jacob Bronowski's stress on the vicious propensities of saturnalia makes the comparison truer. *Face of Violence*, 18–19.

⁵⁵ Testimony of Mr. Blakely, a carpenter, in the third mob case, *Baltimore Gazette*, Dec. 12, 1835.

⁵⁶ Reverdy Johnson, *Memorial to the Legislature of Maryland* (Annapolis, 1836), 8; Brown, “American Vigilante Tradition,” 167–71; Richards, *Gentlemen of Property and Standing*, 131–55. The mobs Brown and Richards ably discuss were more “respectable” than most, but perhaps not generally so “upper class” as they suggest. Richards, for example, bases his key samples on the names of people who attended meetings that resolved basically that violence would occur if abolitionist editor James Birney stayed in Cincinnati or if the New York Slavery Society tried to meet in Utica. Such resolutions could be supported by a wide spectrum of citizens, from those who favored a mob to those who wished to avoid trouble, to those genuinely concerned for the safety of those threatened. That their action as it turned out supported the mob is clear, but that they intended it to do so is dubious. Participants in such meetings did not necessarily countenance riot, much less participate in it. In Cincinnati many citizens must have had in mind Birney's experience in Danville, Kentucky, where a similar public meeting had caused him to leave peaceably.

did some rioting; two of these were carpenters, one a merchant, and one probably a farmer. Testimony revealed the names of nine other rioters, only two of whom were professionally identified, both as carpenters. Thus half of the fourteen rioters identified by job were carpenters and eleven (or 78.6 per cent) were "mechanics," that is workingmen with a particular skill.⁵⁷ No ages were given, but in about one-third of the cases the rioter's youth was mentioned. The evidence is sketchy but corresponds with the usually even less certain data on other riots. Rioters were predominantly lower-middle-class people with a skill or some property and some position in the community; the majority also tended to be young, in their late teens or twenties, and to have ties with the Jacksonian equivalent of the modern urban gang, the fire companies.

In Washington twenty-some persons were arrested, but the press mentioned only two names: John Laub, a ship carpenter from the Navy Yard, and a "Mr. Sweeting, of Philadelphia," possibly of the same vocation. The diary of Andrew Shiner, a black worker at the Navy Yard, offers the most helpful clues about who participated. A large group of out-of-town workers had been hired to refurbish the frigate *Columbia*. Late in July one mechanic was caught stealing copper, and the commander of the Navy Yard, Commodore Isaac Hull, ordered that workers be barred from eating in the storeroom. Considering this order an assault on their honor, the workers went on strike and ten days later eased their offended dignity and relieved their enforced leisure by terrorizing blacks. At some points the mechanics considered attacking the Navy Yard, but prudence and the fortifications kept the riot racial.⁵⁸ The strike explains how mechanics could spend Tuesday afternoon and all day Wednesday working at riot. The clearest evidence that the rioters were of this class grew from a meeting of "very respectable" mechanics called specifically to disavow such ties. The formal resolutions expressed resentment that mechanics should be thought involved with the riot and asked for the removal of federal troops from the streets, but several volunteer amendments, all adopted, revealed more than the meeting's sponsors wished. "Riotous" was changed to "excited," a resolution calling the presence of troops "an insult to freemen" was added, and finally the commander of the troops was damned "for stigmatizing those citizens of Washington who assembled . . . to inflict summary punishment on B. Snow as 'a set of ragamuffins.'" Little wonder that the Jacksonian journal, the *Globe*, an active sponsor of the meeting, had reservations about "the mode adopted to repel" those "unfounded" charges that mechanics had countenanced the riot.⁵⁹

⁵⁷ Job identifications were made from the trial records and *Matchett's Baltimore Directory* (Baltimore, 1835-36). The possibilities of mistake are, of course, rife; the only Peter Harman listed for Baltimore in 1835 was a Lutheran or German Reformed minister, while the riotous Peter Harman was best known as a fireboy among whose favorite phrases were "damned" and "son of a bitch." The evidence against the only convicted laborer was very dubious as to his rioting, although he certainly took home a part of one of John Glenn's carpets.

⁵⁸ Andrew Shiner, diary, 1813-65, ff. 58-61, Library of Congress; Isaac Hull, *Papers of Isaac Hull, Commodore United States Navy*, ed. Gardner Weld Allen (Boston, 1929), 68-77.

⁵⁹ *Globe*, Aug. 19, 1835.

Dreadful Riot on Negro Hill

O Read wid detention de Melancholly Tale and be send you yelling to your bed!



Copy of an intercepted letter from PHILLIS, to her Sister in the country, describing the Riot on Negro Hill.

Fig. 6. A woodcut illustrating a verse broadside in Negro dialect purporting to be a letter from a victim of a Boston riot in the late 1820s to her sister. The riot was ostensibly an attack on houses of prostitution, but became also an assault on the black community in the same area. As was usual in such cases, contemporary newspaper accounts tended to absolve the rioters by emphasizing their moral rather than racial motivation. Illustration from the Library of Congress.

The question of leadership of Jacksonian riots is even harder to answer. In the Snow Riot no evidence remains of leadership, although John Laub was labeled a "ring leader." Those arrested in Baltimore were also called ringleaders, but their trials made clear that only two of them might have been influential even in a secondary way. Yet certainly someone wrote the hundreds of handbills inciting to riot, and witnesses testified that the mob had clear leadership from time to time. The identity of "Red Jacket," "Black Hawk," and "the Man in the Speckled Hat"—names given to alleged leaders—is unknown; perhaps people thought they were leaders only because of their notable costumes.⁶⁰ The riot testimony suggests that Leon Dyer was

⁶⁰ Witnesses identified Samuel Reed as a leader because he "acted like a madman" and Peter Harman as one because he wore a brass plate on his hat and a curtain ring around his neck. *Baltimore Gazette*, Dec. 28, 31, 1835.

active in the crowd; he was not tried because of testimony that he helped prevent destruction at McElderry's. His doing this is not incompatible with being a leader; Benjamin Lynch, one of the convicted, reportedly said during the riot, "Gentlemen, we have gone far enough, if we go further we shall lose the sympathies of the people." Dyer, at any rate, was reported to have said, "I have got the party and can send them where I please," and to have bought drinks for the mob who worked destruction on the McElderry and Ellicott homes. A citizen of Baltimore much later identified "Red Jacket" as "Samuel M. . . , a cooper in Franklin street." This was Samuel Mass, a Jacksonian politician who had been president of the Maryland Executive Council the year before. Mass was arrested for leading a meeting, two days after the riot, of Tenth Ward Citizens who deplored the violence but also warned Reverdy Johnson that he would be deservedly driven out of Baltimore should he have the impudence to return.⁶¹ Dyer and Mass were leaders of the plebian wing of the Jacksonians; when Roger B. Taney went to Annapolis to urge an indemnity for the victims of the riot, Baltimore's Democratic representatives pointedly avoided calling on him, causing Taney to lament that they, like Baltimore's Jacksonian editor, should countenance the political leadership of Leon Dyer and his sort.⁶² The riot occurred because people were generally convinced of the exploitation by the bank's "partners" of the bank's creditors, but possibly the leaders and the most active rioters were lower-middle-class Jacksonians who found in the incident the perfect illustration of Jacksonian rhetoric about the people versus the monied interests, which they took considerably more to heart than did party leaders. When Henry Brown heard in the country that the people were rising up against the "monied aristocrats," he rushed to town, getting there in time to help sack at least John B. Morris's house. When an acquaintance chided Brown because Morris was "the poor man's friend," Brown said had he known that, he would not have hurt Morris's home, but Brown went on railing against the "damned aristocrats."⁶³

⁶¹ Dyer's name was that most frequently brought up in the trials; the testimony suggests he both directed the mob and modified its destructiveness at points. Dyer was born in Germany in 1807 and came with his parents to Baltimore in 1812. His father was the first president of Baltimore's Hebrew Congregation and one of the first beef packers in the United States. First identified in the *Baltimore Directory* of 1842 as a butcher, Leon Dyer was, because of his popularity, appointed acting mayor in the wake of Baltimore's Bread Riot of 1837. See Isidore Blum, *The Jews of Baltimore: An Historical Summary of Their Progress and Status from the Early Days to the Year 1910* (Baltimore, 1910), 9-10. Archibald Hawkins identified Mass as "Red Jacket" in perhaps the best of the historical accounts of the riot. He is the only historian to mention the Bossière affair, and his correctness about this encourages confidence that his identification of Mass may be right. *The Life and Times of the Honorable Elijah Stansbury* (Baltimore, 1874), 90-118.

⁶² Roger B. Taney to James Mason Campbell, Mar. 6, 1836, Benjamin C. Howard Papers, Maryland Historical Society. Taney, who had promised to protect the partners' reputation in return for their earlier support, swung enough Democratic votes to the Indemnity Bill to ensure its passage, partly by getting a statement of support for it from Jackson himself. Taney to David M. Perine, May 28, June 2, June 20, July 10, 1834, Perine Papers; Samuel Tyler, *Memoir of Roger B. Taney* (Baltimore, 1872), 244-45.

⁶³ *Baltimore Gazette*, Dec. 14, 1835. The case illustrated the willingness of American juries

This political situation would explain how Moses Davis, a town drunk, presumably, from the joking newspaper references to him as a "very *spirited* man," got one-fourth of the vote for mayor, to replace Hunt, who resigned. Davis' opponent was the law-and-order candidate, Samuel Smith, who was endorsed by the town's entire power structure from both parties. It would also explain why the vote declined one-third from the previous election despite strenuous attempts to get out the electorate for Smith to salvage "Baltimore's fair reputation." Benjamin C. Howard, one of the city's Democratic congressmen standing for re-election, ostentatiously avoided voting for Smith, despite his friendship for and earlier support of the riot victims. And the political situation would explain the unusual degree of emotive sincerity one senses in this riot's inflammatory handbills.⁶⁴

It is difficult to see much social purposiveness in Jacksonian riots. In Baltimore the riot resulted largely in reaction. The next legislature passed a law making local communities financially responsible for riot damage and an indemnity bill paying the victims of riot fully for their losses out of Baltimore's harbor funds. As the attorneys and leaders of the creditors—who desperately tried to prevent the riot—feared, the incident aided the exploiters by transforming the question of choosing between Johnson and Co. and the creditors to that of supporting Johnson and Co. or the mob. The victimization largely ensured the restored social position of Johnson, Glenn, E. T. Ellicott, and McElderry. None were elected to popular office, but all remained prominent and respected. Reverdy Johnson steered his election to the United States Senate as adroitly through the state legislature as he had his Indemnity Bill and became attorney-general of the United States under Zachary Taylor.⁶⁵ But even had the riot succeeded in ruining or driving out its victims, it would in no way have promoted the relief of those people who lost heavily through the long-continuing trusteeship. The Washington rioters were more successful. Beverly Snow never returned to his nation's capital, no one even considered indemnifying blacks for their losses, and the city council made gestures toward meeting the rioters' demands for more stringent restrictions on free Negroes. The moral is one that runs through Jacksonian riots. Mobs often succeeded in their immediate goals but were in the long run counterproductive when directed against groups or institutions that had some social power. Mormons, Catholics, and abolitionists were all injured by riots, but more fundamentally

to neglect legalism. The jury, after several hours deliberation, requested to ask the leading witness one question: was Brown "very drunk?" The judge said that should have nothing to do with their decision, but added that if they had reasonable doubt of his guilt they should acquit him, which they immediately did.

⁶⁴ *Baltimore Gazette*, Sept. 9, 1835; *Baltimore Republican*, Sept. 9, 1835; Roger B. Taney to James Mason Campbell, Sept. 25, 1835, Howard Papers.

⁶⁵ Bernard C. Steiner, *The Life of Reverdy Johnson* (Baltimore, 1914). The creditors' attorneys urged the avoidance of violence so that their legal case would not be endangered, and one of them, William P. Preston, wrote a personal note to Mayor Hunt on August 9, 1835, urging strong action to prevent riot. William P. Preston Papers, Maryland Historical Society.

Fig. 7 (facing page, above). An illustration of an October 1849 Philadelphia riot that occurred on the evening following an exciting municipal election. The specific object of attack was the California House, a tavern and bawdy house run by a huge black, Hercules, who was married to a white woman. It soon developed into a brawl between different fire companies and urban gangs. Hercules and four or five others were killed, and five buildings were burned. The illustration appeared in an anonymous and romantic account of this riot and of the life of the leader of Philadelphia's most prominent gang in the 1840s, *The Life and Adventures of Charles Anderson Chester, The Notorious Leader of the Philadelphia "Killers," Who Was Murdered While Engaged in the Destruction of the California House* (Philadelphia, 1850).

Fig. 8 (facing page, below). A lithograph of the militia attacking an anti-Catholic mob during a Philadelphia Nativist riot in 1844. During May serious rioting occurred when some Catholics in the Hibernia Hose Company fired into a Nativist meeting. This led to extensive burning and sacking of Irish Catholic homes and religious buildings. Renewed trouble broke out after a Fourth of July parade in which Nativist organizations made much of their "martyrs." Nativist leaders prevented the burning of another Catholic church after arms were found in it, but subsequent altercations between militia and crowd led to an all-night pitched battle in which the mob unsuccessfully used improvised cannon to try to take the building. In the course of the two riots some thirty-five people were killed. Illustration from the Library of Congress.

drew much of their strength from these persecutions. Riots generally succeeded only when directed against the socially defenseless, particularly blacks.⁶⁶

THE AMERICAN IRISH RIOTS illustrate the problem in interpreting Jacksonian mobs as socially purposive. Certainly the Irish had much to be unhappy about, both before and after their coming to the United States. There was some prejudice against them, they had comparatively low-paying jobs, their housing was bad, and they had to send their children to schools tinged with Protestantism. And so Irish rioting could be seen as the just social response of an oppressed group. But as one looks more closely, these riots seem less against the injustices of the system than over traditional religious and clan rivalries and against groups less socially influential than they. Many of the so-called Irish labor riots on the canals, railroads, and aqueducts generally turn out to have been imported clan battles between groups of Irish Catholics from different areas of the old country.⁶⁷ In an instance where they attacked management, records suggest that they were angrier about the foreman's Presbyterianism than his economic exploitation.⁶⁸ It is significant that the Irish participated in riots much more often in New York and Philadelphia where they were quickly welcomed into

⁶⁶ There are two exceptions: the antirent riots of upstate New York encouraged a political solution to an outdated system of land tenure, and the mobs in some areas of the North did rescue a few blacks and help create sympathy for the slave's plight, especially after the Fugitive Slave Law of 1850.

⁶⁷ Especially the Chesapeake and Ohio Canal riots of June 1834 and August 1839; the riot on the Baltimore and Washington Railroad, June 1834; the riots on the Croton Water Works in New York, 1840-41; and the riot on the Erie Railroad near Port Jervis, New York, 1849. Sir George C. Lewis described tellingly how patterns of Irish rioting developed in response to English injustice, but came to be directed, largely per force, against the safer target of other groups of Irish. *On Local Disturbances in Ireland; and on the Irish Church Question* (London, 1836).

⁶⁸ The Washington and Baltimore Railroad Riot in November 1834; see *Niles' Register*, 47 (Dec. 20, 1834): 272.



"A cry at once arose that a white man was shot, and the attention of the mob was directed to the California House, at the corner of Sixth and St. Mary street."—page 30



Engraved according to bill of Congress in the year 1838 by James Beattie in the Clerk's Office of the dist. Court of the Southern dist. of N. Y.

Lithography & Print Coloring on copperplate by James Beattie 1838 James P. N. Y.

RIOT IN PHILADELPHIA

the political system than they did in Boston where they were given no political jobs prior to the Civil War. The Philadelphia riot case is illuminating. Philadelphia's first important postrevolutionary riot occurred in 1825 when a serious brawl broke out between Irish Catholics and Protestants just after they disembarked from the ship that brought them from Ireland; six years later a group of Irish Catholics attacked a parade of Orangemen celebrating the Battle of the Boyne, and a general brawl ensued.⁶⁹ In 1829 the first of a series of eight Philadelphia riots against blacks and abolitionists occurred in which Irish names bulk large among those arrested, though they were obviously abetted by many home-grown rioters, especially in the antiabolition affrays. And the various antebellum railroad, weaver, nativist, fireboy, and antiprostitution riots seem to have had roots in the same ethnic animosities.⁷⁰

Had the oppressed Irish risen over their social hardships against the power structure, Rudé's conclusions about the crowd in history might apply to Jacksonian America, where, instead, riots featured Irish Catholics fighting Irish Protestants, Corkonians attacking Fardowners, and Irishmen harrasing blacks and their supporters. Indeed favorite targets in some anti-black riots were Negro orphan asylums, homes for perhaps the most hapless of American citizens.⁷¹ The sad truth about the Jacksonian riots was that, though the performers had real grievances and fears, action was generally taken only when there was large promise of safety: by groups in situations and places where they had fairly broad political and social influence and against individuals and groups less popular than they.⁷²

Jeremiah Hughes's analysis of both the source and social effects of Jacksonian rioting was well taken:

A radical error in democratic ethics begins to develop itself. The people have been told so often that all power, government, and authority of right belong to

⁶⁹ David Paul Brown, *Speech Before the Mayor's Court of Philadelphia, September 17, 1825, on the Subject of Riot and Assault and Battery* (Philadelphia, 1858); *A Full and Accurate Report of the Trial for Riot Before the Mayor's Court of Philadelphia on the 13th of October, 1831, Arising out of a Protestant Procession on the 12th of July in Which the Contending Parties Were Protestants and Roman Catholics* (Philadelphia, 1831).

⁷⁰ The many nativist riots, commonly seen as part of a "Protestant crusade" against Catholicism, had roots in much more complicated ethnic, religious, and social animosities in urban Jacksonian America. An able exploration of these many strands is William Baughin, "Nativism in Cincinnati Before 1860" (master's thesis, University of Cincinnati, 1950).

⁷¹ The Philadelphia Abolitionist Riot, May 1838; the New York Draft Riot, July 1863. The orphanage attacks allowed release of social anger not only against that group the rioters were determined to keep as social inferiors, but also against their philanthropic social "betters," who endowed the institutions and whom it would have been dangerous to attack directly.

⁷² Jacksonian riots could be fitted to the schemas social scientists have worked out for explaining civil disturbances, but this owes perhaps more to the flexibility of various models than their explanatory usefulness. "Social disequilibrium," the "expectation gap," the "J-curve of rising and declining satisfactions," and "relative deprivation" are sufficiently vague to be discoverable wherever sought. Chalmers Johnson, *Revolutionary Change* (Boston, 1966), 59-87, 119-34; Ivo K. Feierabend, Rosalind L. Feierabend, and Betty A. Nesvold, "Social Change and Political Violence: Cross National Patterns," and James C. Davies, "The J-Curve of Rising and Declining Satisfactions as a Cause of Some Great Revolutions and a Contained Rebellion," both in Graham and Gurr, *History of Violence*, 632-730; Gurr, *Why Men Rebel*, 3-91, 317-59. Jacksonian riots clustered in periods of general prosperity, the mid-1830s, 1840s, and 1850s.

them and that they in fact are the only sovereigns here, that it is not to be wondered at that they occasionally mistake the true limit of that sovereignty, and undertake to exercise despotic powers. Who dare control the *People, a Free People*? Don't they make the government itself, and can't they rule it as they please? Such to a great extent is the political education of the day. . . . Governments are instituted mainly for the protection of the weak from the power of the strong. But for this they would not be endured. The majority are always powerful—they require no protection. To restrain an undue exercise of power against the weak is one great motive for which government is instituted.⁷³

Hughes's concern about the weak, about minorities, is very much to the point. Victims, more than rioters, were the oppressed, the unpopular, the unprotected.

The psychological effects of rioting are even harder to gauge than its social results. Mobs when unopposed clearly enjoyed themselves; two Baltimore rioters said that they got their \$100 and \$500 worth of enjoyment—presumably sums lost to the bank—out of their night's work. The amorphousness of bourgeois-democratic society and the constant Jacksonian stress on power belonging to the people made attractive the sense of group identity and invincibility that came from being part of what John Quincy Adams called "the mobility." A song recorded by a Campbellite minister and temperance lecturer who led the Hancock County anti-Mormon mob in Illinois caught some of the "togetherness" of the riotous crowd: "Hancock is a beautiful place / The Antis all are brothers. / And when one has a pumpkin pie / He shares it with the others."⁷⁴ Democracy's mythic heroes stand outside of society; most of the people who idolize them are enmeshed in it and, if Tocqueville and others are right, have strong desires to merge entirely with the mass.⁷⁵ The psychological appeal of riot in democratic society is that the situation gives a sense of acting by a higher code, of pursuing justice and possessing power free from any structural restraint, and at the same time allowing a complete absorption in the mass so that the individual will and the social will appear to be one. To riot is to be Natty Bumppo in crowd, to be Randolph Scott en masse—and this is a kind of apotheosis for democratic man, fulfilling the official doctrine that power belongs to him and allowing him to escape the real system that attempts to share influence by making everyone powerless.⁷⁶ The most famous rioter

⁷³ *Niles' Register*, 66 (July 27, 1844): 344-45.

⁷⁴ John Quincy Adams, *Memoirs* (Philadelphia, 1876) 9: 252; Thomas Brockman to Andrew Johnston, Jan. 1, 1847, in the Mormon Collection, Chicago Historical Society.

⁷⁵ Tocqueville, *Democracy in America*, 2: 109-13, 334-39; Erich Fromm, *Escape from Freedom* (New York, 1965), 17-38, 157-230; Hannah Arendt, *The Origins of Totalitarianism* (New York, 1958), 305-39.

⁷⁶ Hannah Arendt's distinction between power based on concerted popular acquiescence and violence based on instrumental force is pertinent here, although perhaps a more telling contrast is between power, the essence of which Bertrand de Jouvenal defines as the ability "to command and to be obeyed," and influence, which is the ability to dictate action only through an ongoing process of convincing or manipulating others to agree with a particular policy. Arendt, *On Violence* (New York, 1970), 35-56; Jouvenal, *On Power: The Nature and History of Its Growth* (Boston, 1948), 96. Louis Hartz suggests the vacuousness of the Jacksonian stress on "the will of the people" because the idea promised a direct power that

of the Jacksonian period was also the prime developer of the popular Western story.⁷⁷ The permanent value of such mental satisfactions is less certain. Psychologically as well as socially, perhaps, people who associated themselves with groups victimized gained most from riot, if their groups were not permanently oppressed by it.⁷⁸

THE PROBLEM OF RIOT CONTROL in the Jacksonian period centered in a democratic sense of the limitations on the state's right to use strong physical force against the people. Five people died in Baltimore because the guard did get reluctant permission to fire, although many citizens felt that all trouble would have vanished if the guard had been properly armed in the first place and that fact had been made known. Total peace returned when Smith organized his heavily armed patrols, but by this time the use of force was supplemented by a revulsion of feeling against the mob, particularly when it was learned that a large list of additional victims had been designated.⁷⁹ On the first day after the Washington riot began, the militia was seemingly instructed to try to awe the mob but not to interfere very actively if assaults were confined to black property. When Jackson returned to the city his strategy became one of conciliating the rioters while keeping enough troops around to prevent serious damage. Andrew Shiner was obviously repeating gossip but described Jackson's method accurately.

When this great excitement commenced the Hon. Major General Andrew Jackson that wher president . . . wher absent from the City and when it got in it height the general arrived home and after he arrived home he sent a message to those gentlemen Mechanics to know what was the matter with them and if they were anny thing he could do for them in an Hon. way to promote their happiness he would do it.

When they complained of Negro actions, Jackson assured them "by the eternal god in this city" he would personally see that the blacks were punished if the mechanics had any disclosures to make about illegal activities, but he made clear "by the eternal god the law must be preserved at the Risk of Hasards." Minor sporadic incidents occurred later, but in a

inevitably created distaste for the realities of the political system meant to embody it. *Economic Policy and Democratic Thought: Pennsylvania 1776-1860* (Cambridge, Mass., 1948), 23-33, 309-20.

⁷⁷ E. Z. C. Judson was convicted of instigating the Astor Place Riot of 1849 in New York and was indicted for his part in a St. Louis election riot of 1852. As "Ned Buntline," he also was the leading developer of the dime novel. Jay Monaghan, *The Great Rascal: The Life and Times of Ned Buntline* (New York, 1952).

⁷⁸ Helpful studies of the psychological sources and effects of violence are Hans Toch, *Violent Men: An Inquiry into the Psychology of Violence* (Chicago, 1969); Silvan S. Tomkins, "The Psychology of Commitment: The Constructive Role of Violence and Suffering for the Individual and for His Society," in Martin B. Duberman, ed., *The Anti-Slavery Vanguard: New Essays on the Abolitionist* (Princeton, 1965), 270-98.

⁷⁹ Joint Committee of the Maryland General Assembly on the Baltimore Riots, *The Report of and Testimony Taken Before the Joint Committee of the Senate and House of Delegates of Maryland* (Annapolis, 1836); *Baltimore American*, Aug. 12, 1835; "Junius" in the *Baltimore Republican*, Mar. 5, 1836.

couple of weeks Washington "was as quiet as a church and the laws wher all respected."⁸⁰

Outside of Washington, the multileveled character of American government kept riot control largely a local problem to be coped with by local officials. Such people, even more than Jackson, were often understandably sympathetic to their fellow citizen-voters or at least hesitant to attack any large group of them. Hence there was much truth to the frequent assertion that a greater show of determination on the part of authorities would have proved effective in stopping trouble. Some observers considered even a real show of determination an inadequate response to threatened violence. Roger Taney complained that the Baltimore bank mob ought to have been met by a "firm and free" use of guns at once, and Wendell Phillips accused the Boston mayor of being derelict in his duty for not having "ten men shot and sent to deserved graves" in the Garrison mob—this in a riot where the mayor acted with vigor and personal courage and where the total estimated damage was fifteen dollars.⁸¹ Even gross dereliction of duty was perhaps better than Phillips's emotive and moralistic approach, which could only feed the paranoiac self-righteousness rioters, actual and potential, possess. The heaviest loss of life tended to occur in two riotous situations: when authorities wholly acquiesced in a mob's destructive tendencies—as was the case with the Mormons in Missouri, with some groups of gentiles later in Mormon-controlled Utah, and in alleged abolitionist and slave conspiracies in the South; or when force was used to keep a mob from their ends. Elijah Lovejoy would not have been killed if he had let his press be removed from Alton as he had from St. Louis. No one died at the Ursuline Convent, which the mob burned unopposed; twenty were victims of the military when the mob was not allowed to fire a Philadelphia Catholic church. How weigh the five bodies in Baltimore against the property of the bank partners or that of men whose crime was answering a public call to aid in keeping the peace? How put Beverly Snow's small property and dignity in the balance with the lives that it might have cost to protect them? In some cases even human life may be less important than using force, if absolutely necessary, to allow unpopular faiths to be followed, unpopular people to be protected, unpopular ideas to be heard.

The clearest result of Jacksonian rioting was the development of professional police and fire companies in large cities.⁸² In the wake of the Balti-

⁸⁰ Shiner, diary, ff. 60-61. At least some damage to black property occurred as late as mid-September, although the major Washington papers did not mention it. *Baltimore Gazette*, Sept. 15, 1835; *Niles' Register*, 49 (Sept. 19, 1835): 33.

⁸¹ Frank Otto Gatell, "Roger B. Taney, the Bank of Maryland and a Whiff of Grapeshot," *Maryland Historical Magazine*, 59 (1964): 262-67; Theodore Lyman III, ed. *Papers Relating to the Garrison Mob* (Cambridge, Mass., 1870), 7.

⁸² Roger Lane, *Policing the City: Boston 1822-1885* (Cambridge, Mass., 1967), 26-38; James F. Richardson, *The New York Police, Colonial Times to 1901* (New York, 1970), 28-30; Sam Bass Warner, *The Private City: Philadelphia in Three Periods of Its Growth* (Philadelphia, 1968), 125-57; Andrew H. Neilly, "The Violent Volunteers: A History of the Volunteer Fire Department of Philadelphia, 1736-1831" (Ph.D. dissertation, University of Pennsylvania, 1960).

more riots there was a strong recognition of the lack of organized civil authority to cope with such problems. The only "republican solution" seemed to be organization of volunteer peace-keeping forces because a professional "army" to ensure order among the people was certainly a mark of despotism. City guards were formed in each Baltimore ward, and gout-ridden Henry Thompson headed a corps of City Horse Guards, but such organizations, without the stimulus of any very urgent business, quickly waned. At the same time, Sir Robert Peel's organization of the London police suggested that a professional police force need not be despotic and pointed to the solution that Americans would accept in the 1840s and 1850s.⁸³ Riots, along with the increasing problem of crime, made clear in urban areas at least that the old voluntary principle could no longer handle social control among a people growing, and growing apart in economic status and ethnic diversity. American democracy, very reluctantly, came to accept that order and freedom required not only a legal system, but professionals specifically responsible for upholding it and forcing its dictates on recalcitrant fellow citizens. If Andrew Jackson was a political symbol for the mythic anarchic American ensuring the triumph of a higher code by his own strength and integrity in a world neatly divided between virtuous men and monstrous enemies, Abraham Lincoln came to represent the sadder side of the democratic psyche: the need to assert man's potential for freedom through accepting cruel responsibilities for using force in a world where the morality of all men was a mixed bag and where both sides prayed to the same God. Lincoln in his famous law-and-order speech of 1837 used recent riots to argue that only in unswerving respect for the law lay real protection from vicious disintegration and despotism. He and his nation in the Civil War proved their willingness to insist on their conception of law even if it had to be imposed by military force.⁸⁴ In his *Battle-Pieces*, Herman Melville, the American who had most developed the theme of the heroically destructive potential of self-reliant individualism, noted how the Civil War marked society's tacit acceptance of his grimmer vision of man's fate—especially in a poem commenting on New York City's Draft Riot of 1863 where for the first time a professional police force was used not to control but to conquer "the Atheist roar of riot":

Hail to the low dull rumble, dull and dead,
And ponderous drag that shakes the wall.
Wise Draco comes, deep in the midnight roll
Of black artillery; he comes, though late;
In code corroborating Calvin's creed
And cynic tyrannies of honest kings;

⁸³ *Baltimore Gazette*, Sept. 13, 19, 1835; *Baltimore American*, Aug. 27, Sept. 12, 1835; Thompson, diary, Sept.–Oct. 1835.

⁸⁴ Abraham Lincoln, "Address Before the Young Men's Lyceum of Springfield, January 27, 1838," in *Collected Works*, ed. Roy P. Basler (New Brunswick, 1953), 1: 110–12; Harry Jaffa, *The Crisis of the House Divided: An Interpretation of the Lincoln-Douglas Debates* (New York, 1959), 183–232.

He comes, nor parlies; and the Town, redeemed,
 Gives thanks devout; nor, being thankful, heeds
 The grimy slur on the Republic's faith implied,
 Which holds that Man is naturally good,
 And—more—is Nature's Roman, never to be scourged.⁸⁵

Still unshaken in their democratic convictions, Americans admitted in their prosecution of the Civil War and their growing resistance to rioters that the nation was in practice willing to temper the democratic myth of social responsibility through freedom with some of Draco's stern legalism and Calvin's harsh estimate of man's character and destiny. With a willingness, if you will, to use law not only to release human energy but to check and control it.

The Jacksonian experience suggests that riot is not antithetical to, or abnormal in, a democracy but the result of very basic tendencies and tensions within it. Because of these the riot situation poses in stark form many of the deepest dilemmas a democracy faces. To react harshly is to threaten groups who act within its bounds and in accord with some of its basic precepts; to react tolerantly is inevitably to make the state an accomplice in whatever is done. Riot crystallizes the paradox of vital democracy that must live in the shadow of twin totalitarianisms—that of total submission of all to the state's power and that of the tyranny of favored groups or individuals because of the state's weakness. And to avoid the ascendancy of either totalitarianism requires that democratic man live uneasily and creatively with the dangerous proclivities, potential and sometimes realized, in both his legalistic and anarchic myths.

⁸⁵ Herman Melville, "From the Roof Tops," *The Battle-Pieces*, ed. Hennig Cohen (New York, 1963), 89–90. The change in attitude toward the power of the state paralleled other intellectual shifts, some of which are traced in George M. Frederickson, *The Inner Civil War: Northern Intellectuals and the Crisis of the Union* (New York, 1965), in R. Jackson Wilson, *In Quest of Community* (New York, 1968), and in Hurst, *Law and the Conditions of Freedom*.

The Stem Family and the Developmental Cycle of the Peasant Household: An Eighteenth-Century Austrian Example

LUTZ K. BERKNER

THERE HAS BEEN a widely held sociological theory that the nuclear family structure is linked to the development of modern Western society.¹ By "nuclear" is meant a man, his wife, and their unmarried children, if any; this becomes "extended" if their household also includes married children or some other kin. It is commonly thought that kinship ties were stronger in the past, but it has never been very clear exactly from what previous form the modern nuclear family has emerged. This is hardly the fault of sociologists, who until recently could turn to virtually no historical family studies that would meet the standards of modern scholarship.² Only in the last few years have we seen an increase in the number of serious works on the family by historians, and we now have some excellent regional monographs³ and one general survey, Peter Laslett's *The World We Have Lost*.⁴ From his statistical studies of early censuses, Laslett has concluded that in England the nuclear family has always predominated and that there is so little evidence of extended or stem families in the past that theories claiming a shift to a nuclear pattern in modern times cannot be empirically validated.⁵

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¹ William J. Goode, *World Revolution and Family Patterns* (New York, 1963) is a comprehensive survey of this thesis with many important qualifications, some of which are summarized in the preface to the paperback edition (1970).

² As was pointed out by William J. Goode, *The Family* (Englewood Cliffs, 1964), 105.

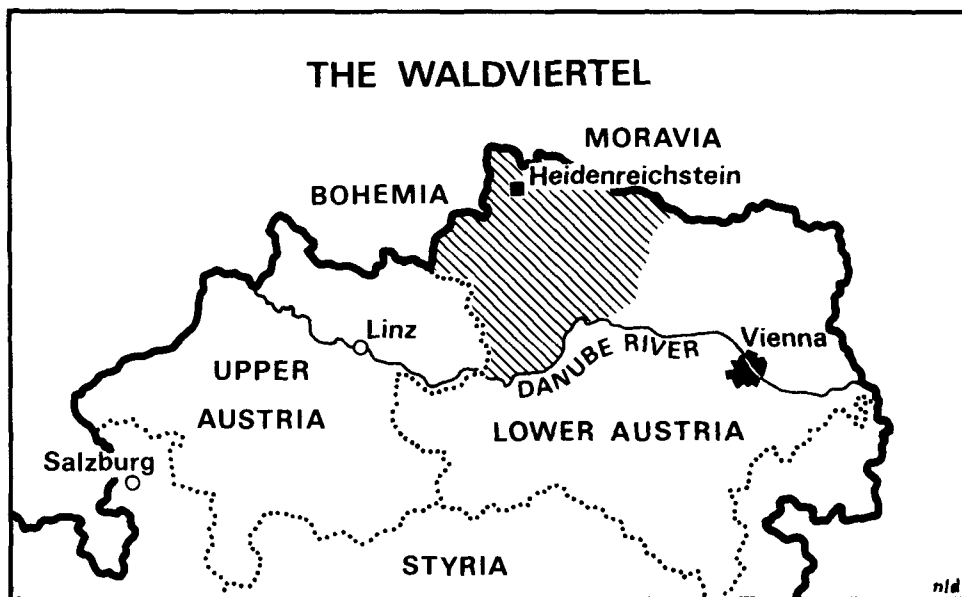
³ For example, Philip J. Greven, Jr., *Four Generations: Population, Land, and Family in Colonial Andover, Massachusetts* (Ithaca, 1970); John Demos, *A Little Commonwealth: Family Life in Plymouth Colony* (New York, 1970); Michael Drake, *Population and Society in Norway, 1735-1865* (Cambridge, 1969); Anne Zink, *Azereix: La vie d'une communauté rurale à la fin du XVIII^e siècle* (Paris, 1969); and the too-little-known masterpiece by Rudolf Braun, *Industrialisierung und Volksleben: Die Veränderungen der Lebensformen in einem ländlichen Industriegebiet vor 1800 (Zürcher Oberland)* (Erlenbach-Zürich, 1960), a portion of which has been translated by David Landes (David Landes, ed., *The Rise of Capitalism* [New York, 1966], 53-64).

⁴ Peter Laslett, *The World We Have Lost* (New York, 1965; 2d ed., London, 1971). Page references are to the first edition.

⁵ *Ibid.*, 89-92. This has been restated with additional evidence in Peter Laslett, "Size and Structure of the Household in England Over Three Centuries," *Population Studies*, 23 (1969):

Before we accept this revision as true for Western Europe as a whole, I would like to add two notes of caution: first, that the stem family did and does exist as an important part of the social structure in many parts of rural Western Europe,⁶ and second, that the stem family structure does not necessarily emerge from empirical studies of demographic statistics unless the developmental cycle of the family and household are taken into consideration in the analysis. This will be illustrated by an examination of the eighteenth-century peasant stem family in the Waldviertel, a region of Lower Austria bordering on Bohemia and Moravia.

The term stem family was coined by the sociologist Frédéric Le Play to describe a specific type of extended family organization in which only one child marries while remaining at home to inherit the family property and



the other children either leave to establish their own families elsewhere or stay in the household as celibates.⁷ Although Le Play assumed that the

199-223. The same point, illustrated with statistical evidence from Switzerland, was made earlier by Emil J. Walter, in "Kritik einiger familiensoziologischer Begriffe im Lichte der politischen Arithmetik des 18. Jahrhunderts," *Schweizerische Zeitschrift für Volkswirtschaft und Statistik*, 97 (1961): 64-75.

⁶ Conrad Arensberg and Solon Kimball, *Family and Community in Ireland* (Cambridge, Mass., 1940), is probably the most often cited description of a stem family system. Zink, *Azereix*, 234-40, describes the stem family in the French Pyrenees in the eighteenth century and Pierre Bourdieu, "Célibat et condition paysanne," *Études rurales*, nos. 5-6 (1962): 32-135, discusses the same area in more recent times. Jan Stehouwer, "Relations Between Generations and the Three-Generation Household in Denmark," in Ethel Shanas and Gordon F. Streib, eds., *Social Structure and the Family: Generational Relations* (Englewood Cliffs, 1965), 142-62, presents some contemporary statistics on the frequency of three-generation families in Europe.

⁷ Frédéric Le Play, *L'Organisation de la famille* (Paris, 1871), is the classic work. A historical application of Le Play's family types can be found in George C. Homans, *English Villagers of the Thirteenth Century* (Cambridge, Mass., 1941), 109-222.

father would either maintain or share the ownership of the family farm until he died, he could also transfer the property to the heir and retire. In Austria it was retirement that formed the cornerstone of the stem family organization.

In 1763 the manor of Heidenreichstein in the Waldviertel took a census of households in its thirty-six villages.⁸ Heidenreichstein was a small market town of ninety houses and a manorial castle, surrounded by a scattering of very small peasant villages that rarely contained more than thirty houses. This was an area of concentrated nuclear villages and open fields, although at the end of the seventeenth century some new scattered settlements appeared. Although 729 households belonged to the manor, we will only be concerned with the 651 (ninety per cent) "peasant" households. By "peasant" (*Bauer*) we mean the property-holding head of a farming household who was regarded as "neighbor" (*Nachbar*) in the community, a person who enjoyed political rights in the village and economic privileges in the common lands and forests.⁹ This excludes servants and lodgers, who, as we shall see, were considered part of the peasant's household, as well as 51 households of landless laborers living in "common houses" owned by the village and 27 households of persons who were not directly involved in peasant farming.

Based on an analysis of the peasant households enumerated in this census, what follows will examine first the life cycle of an individual peasant householder, then the extended family, and finally the composition of the entire household of peasants, servants, and lodgers.

THERE WERE THREE major stages in the life cycle of the peasant householder: son and heir, head of the household (*Hausvater*), and retired parent (*Ausnehmer*). According to the land laws, the peasant holding (*behaustes Gut*) was impartible, but the inheritance customs in Lower Austria insured equal division of family property among all the children. Only one of them, the heir chosen by the parents, actually received the house and the land while the other children were compensated in money payments. Since the holding was usually held as common property by the man and his wife, when one of them died the other retained half of the property and the children divided the value of the other half, which they held as legal claims on the farm. The heir became a householder by making

⁸ This census, which is the basis of all the statistics in this article, is in the private Schlossarchiv Heidenreichstein, 13/1 (*Seelenbeschreibung* 1763). I wish to thank Graf Christian Kinsky for his kind permission to use this archive, which is located in the tower of the old castle in the town of Heidenreichstein.

⁹ Otto Brunner, *Land und Herrschaft: Grundfragen der territorialen Verfassungsgeschichte Österreichs im Mittelalter* (4th ed.; Vienna, 1959), 256–59; Helmuth Feigl, *Die niederösterreichische Grundherrschaft vom ausgehenden Mittelalter bis zu den theresianisch-josephinischen Reformen* (Vienna, 1964), 122–43.

a settlement with the surviving parent and assuming the debts caused by his brothers' and sisters' claims.¹⁰

Often an elderly peasant couple would sell the farm to the heir and go into retirement (*ins Ausnahm gehen*). In some areas of Austria there was a special little house set aside for the retired parents; in the Waldviertel a room (*Stübl*) was generally built onto the house.¹¹ A legal contract of sale effected the transfer between parents and heir. The amount to be paid was usually staggered to ease the burden of payment and to provide an annual income for the retired parents. The contract was full of specific rights and privileges reserved for the parents as long as they lived. These included the right to live in a room rent-free, the use of certain pieces of land, and the provision of specified amounts of grain, fodder, and wood every year.¹²

When, for example, Joseph and Anna Maria Pichler decided to retire in October 1784 they drew up a contract with their son Johann and his bride Gertraud, selling them their house and fields for 100 florins. Joseph deducted 20 florins from the price as a wedding gift to his son and asked that the rest be paid in installments of 20 florins every *Michaeli* (September 29). As a retirement settlement (*Ausnahm*) they reserved the right to live in the *Stübl* rent-free for the rest of their lives, the use of a small piece of meadow and a section of the garden to grow cabbage and potatoes, and a yearly supply of seven bushels of wheat, thirty-two batches of hay, and two piles of wood.¹³

Why should a father make such specific demands and insist on their being put into a legal contract when he was dealing with his own son? As an *Ausnehmer*, the parent relinquished all legal authority over his farm. The reservations in the contract were all he had left of his former possessions. And the only guarantees he had to these rights and to the payments due him were the good will of his son and a legal contract with very specific details in it. The fact that the peasants invariably chose to include the details of their rights in the contract suggests a common-sense awareness of the frailty of such agreements without any legal guarantees. The classic literary illustration for this concern is Emile Zola's *La Terre*, in which a peasant partitions his lands among his sons, expecting them to support him in his old age. One after another the children fail to live up to

¹⁰ Helmuth Feigl, "Bäuerliches Erbrecht und Erbgewohnheiten in Niederösterreich," *Jahrbuch für Landeskunde von Niederösterreich*, n.s. 37 (1965-67): 161-83; Feigl, *Grundherrschaft*, 66-73.

¹¹ Heinrich Rauscher, "Volkskunde des Waldviertels," in Eduard Stepan, ed., *Das Waldviertel*, 3 (Vienna, 1926): 11; Arensberg and Kimball, *Family and Community*, 135, note that in Ireland a "west room" was set aside for parents at their retirement.

¹² The contracts, many of which are available in the Niederösterreichische Landesarchiv (hereafter NOLA), were copied into the *Kaufprotokolle* kept by the manor. There were similar contracts in other countries. For France, see Patrice Higonnet, *Pont-de-Montvert* (Cambridge, Mass., 1971), 21-22, 49; for colonial Massachusetts, see Greven, *Four Generations*, 84, 95, 143-44, 242-43.

¹³ NOLA, B.G. Litschau, Herrschaft Heidenreichstein, no. 66 (Kaufprotokolle 1774-86), fol. 395.

the agreement, refuse to make their payments to the father, and ignore the wishes of the man who has lost his authority and is now at the economic and physical mercy of his ungrateful children.

There must have been severe psychological strains within the peasant family after the change of ownership. The very existence of separate living quarters, the *Stübl*, is an indication of the conscious awareness of the tension that was bound to develop between the old parents, stripped of the economic power and the authority that they had always had over their children, and the young couple, exercising their independence for the first time.¹⁴ The retiring peasants in the Waldviertel had no illusions about the tranquility of future relations with their children. When Michel and Maria Redl in Taures retired in 1779 they reserved the usual right to free lodging within the house they were selling to their son and his wife, but in the contract it was noted that "if they cannot agree with one another, then they would build a separate room onto the house."¹⁵ A similar phrase, or a clause stipulating that if the parents could no longer live in the house the son would pay for their rent elsewhere, was often written into the contracts.¹⁶

Eighteenth-century family relationships among the Waldviertel peasants were probably no different from those described by a local folklorist in 1926:

Although people say "Respect your Elders," when the parents go into retirement they are often not treated the way they should be. The fault usually lies on both sides. The "Old Ones" often don't like the daughter-in-law or the way the young ones run the farm. The "Young Ones" in return don't always quietly accept the grumblings of the "Old Ones" and once a strained relationship has developed, it usually cannot be overcome. The young ones who take over the farm with debts on it also find the retirement settlement very burdensome, for they must still "pay off" their parents and siblings. The old ones are not very considerate in their demands. Although they help out with the work, the need to hire servants adds another financial burden. From all of this we see that the young peasant "ain't got nothin' to laugh about."¹⁷

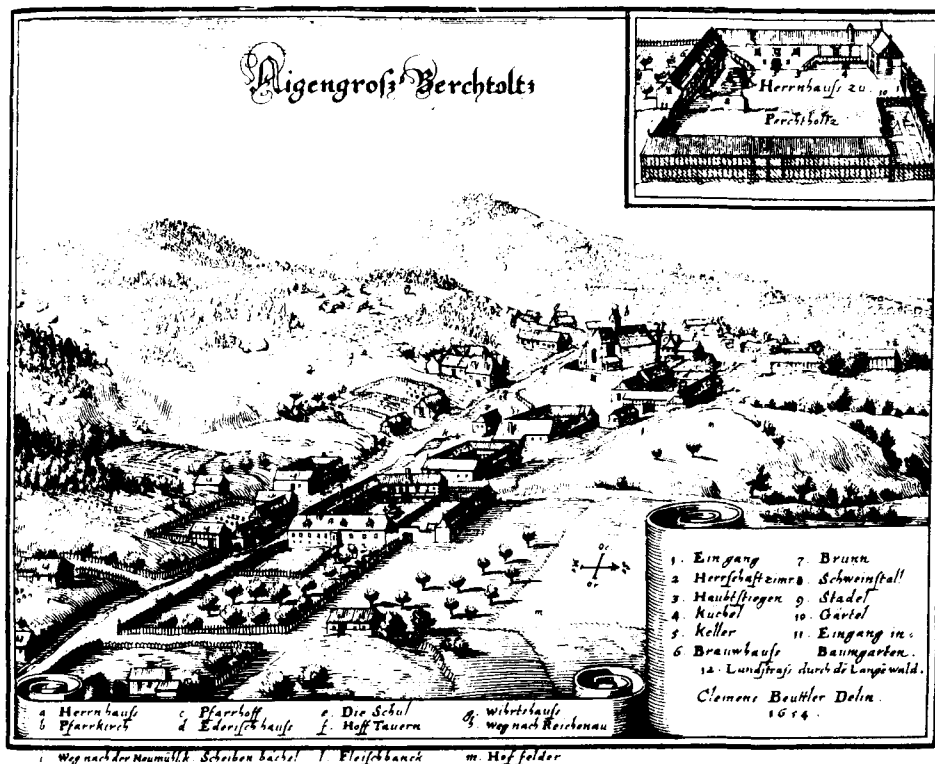
If retirement really meant so much trouble, why was it such a common practice? As the peasant got older and his physical capabilities declined, the option of exchanging the drudgery of hard farm labor for a small fixed income undoubtedly looked increasingly more desirable. At the same time he was feeling rising pressures from his son the heir who was becoming old enough to marry and head the household and who wanted to start doing things right for a change. Since the heir had no source of income he could

¹⁴ Peter Rosegger, *Volksleben in Steiermark* (Leipzig, 1914), includes a chapter, "Das Ausnahmshäusel," that gives a good picture of the psychological tensions involved in peasant retirement in Austria. Arensberg and Kimball (*Family and Community*, 115-40) discuss the same problem in Ireland, Zink (*Azereix*, 234-48), in southern France, and Greven (*Four Generations*, 133-38), in colonial Massachusetts.

¹⁵ NOLA, B.G. Litschau, Herrschaft Heidenreichstein, no. 66 fol. 187.

¹⁶ *Ibid.*, fol. 127. Zink, *Azereix*, 235, 237, quotes comparable stipulations in French contracts.

¹⁷ Rauscher, "Volkskunde," 22.



A seventeenth-century sketch of Gross Perchtoltz, a village in the northwestern Waldviertel near the Upper Austrian border. The quadrangular farmhouses situated in a line along both sides of a single street are typical for the whole region. The large farmhouse detailed in the insert, with its own brewery and somewhat pretentious formal garden, is the seat of a petty manor with rights over seven peasant holdings in the village. From Caspar Merian, *Topographia Windhagiana* (Frankfurt a.M., 1656), pt. 2 between pages 2 and 3, which is bound as "Anhang III" in Mathaeus Merian, *Topographia Provinciarum Austriacarum* (Frankfurt a.M., 1649; reprint Kassel, 1963).

not get married without the parents' permission, which was ordinarily not granted while the father remained head of the household. The sons therefore either waited for the father's retirement or left home to establish their own households. This conflict is expressed in some folksongs that were sung in the Waldviertel at the beginning of this century.

Voda, wann gibst ma denn's Hoamatl,
 Voda, wann loszt ma's vaschreibn?
 s'Dirndl is gwoxn wia's Groamatl,
 Lede wülls a nimmer bleibn.

[Father, when ya gonna gimme the farm,
 Father, when ya gonna sign it away?
 My girl's been growin' every day,
 And single no longer wants to stay.]

Voda, wann gibst ma denn's Hoamatl,
 Voda, wann gibst ma denn's Haus,

Wann gehst denn amol in dein Stüberl ein,
Und grobst da bra Eräpfoln aus?

[Father, when ya gonna gimme the farm,
Father, when ya gonna gimme the house,
When ya gonna retire to your room out of the way,
And dig up your potatoes all day?]¹⁸

If a peasant normally married around age 25 and a son who would live to adulthood was born within the next three years, this son would be ready to marry and take over the farm when his father was in his late fifties, and the farm would change hands every thirty years or so with a minimum of domestic friction. This seems in fact to have been the usual pattern.¹⁹ But many householders died before a son was old enough to marry, and then the widow would either run the farm by herself or remarry. Given the high mortality of the century, remarriage must have been rather common, since married couples headed ninety-six per cent of the 651 households. The rest were headed by 19 widows, 5 widowers, and 2 young unmarried men. While there were still young children in the family, both widows and widowers were likely to remarry. But when there was a son old enough to take over the farm, a widow would probably choose to retire whereas a widower would probably remarry.²⁰

Simon Riener retired in 1768, when he was 55 years old, after having worked his farm in the village of Eberweis for thirty-three years. Both of his daughters had already married and his son, Johann, who was only 20, had just picked a bride. Simon's farm was worth 200 florins when he owned it; three years after he transferred it to his son, Simon died with only 10 florins to his name. Sebastian Böhmesser, who lived across the street, died when his son Leopold had already turned 19. But instead of letting him take over, the widow Maria Böhmesserin kept title to the farm for six years. When Leopold married at age 25, she sold him the farm for only 40 florins, although it was worth 200. Matthias Küpfel, a few houses away, was not so lucky. His father held onto the farm until he was 65, and Matthias did not marry until he was in his late thirties.²¹

In fact only half of the peasants over the age of 58 recorded in the 1763 Heidenreichstein census had retired; half of them were still active heads of households. Three-quarters of these (46 out of 61) either had no sons

¹⁸ *Ibid.*, 99.

¹⁹ Based on a number of family histories put together from the Dienstbuch, the Kauf-, Inventur- and Heiratsprotokollen, and the Theresianische Fassion for Heidenreichstein, all in the NOLA.

²⁰ There were eight times as many widows as widowers under the age of 58. A similar custom of peasant retirement and the resulting high ratio of widows to widowers is noted in Drake, *Norway*, 116.

²¹ NOLA, Theresianische Fassion 870 (Heidenreichstein), fols. 409, 401, 412; NOLA, B.G. Litschau, Herrschaft Heidenreichstein, no. 1 (Dienstbuch, 1715-74), fols. 409, 401, 412, no. 101 (Inventursprotokolle, 1754-74), fol. 307.

or only had sons under 25 living in the household, so the pressure to retire had not yet reached its peak. Although the majority of older peasants conformed to the normal pattern of giving up the farm by the time the heir was 25, seven peasant householders lived with a married son or daughter who had not yet taken over ownership of the farm. These men, as well as the eight peasants who were living with unmarried sons between 30 and 40, were no doubt feeling strong pressure to retire.

We are rather far from the romantics' picture of the happy extended family of father and married son, working side by side and sharing a common income. When the son married, the father usually relinquished control of the farm. At its transfer he retired and worked only the garden plots he had reserved for himself. The son managed the farm alone but furnished a specific amount of goods and money to his parents every year, not necessarily out of devotion but because he had contracted to do so. They lived in the same house, but usually in separate quarters. And if there were no separate accommodations, the agreement often provided for building them if and when the family could no longer live together in harmony.

FAMILIES GO THROUGH developmental cycles as the individuals who compose them go through their life cycles. A census taken at a given point in time takes a cross-section and gives a static picture of households and families that the historian or sociologist can sort out into types. We can count so many extended families, so many nuclear. But rather than being types these may simply be phases in the developmental cycle of a single family organization. There may be a normal series of stages that appear only rarely in a population because they last for only a short period of the family's cycle or in some cases do not appear at all. From this point of view, the extended family is merely a phase through which most families go.²² Since there is a good chance that the parents will still be alive, when a young couple marries, they begin their marriage in an extended family. In time the parents die, and the now middle-aged couple spend their years in a nuclear family. When one of their sons marries and brings his wife into the household the family becomes extended again. And so forth. When a census reveals a low frequency of stem families it is tempting to conclude that stem families are rarely found in the society at any period in time. But it is just as possible to conclude that most families simply happen to be in the nuclear phase at the particular time the census is taken. In fact modern demographic studies have shown that the nuclear phase predomi-

²² Meyer Fortes's introduction to Jack Goody, ed., *The Developmental Cycle in Domestic Groups*, Cambridge Papers in Social Anthropology, no. 1 (Cambridge, 1958), is an excellent discussion of this fundamental concept, which is also touched on by Goode, *World Revolution*, 371.

nates in most societies, and this is the reason why the extended family household is seldom found as the modal or average type in any population.²³

In this Waldviertel census of 1763 only 25 per cent of all peasant households included any kin and could be considered extended. But if we examine the type of family while controlling for the age of the head of household,²⁴ the extended family emerges as a normal phase in the developmental cycle of the peasant household (see table 1). In the households headed by a man between 18 and 27, 60 per cent are extended families, in those headed by men 28 to 37, 45 per cent are extended. Then the percentage of families with retired parents or celibate siblings drops sharply, and the

TABLE 1. TYPE OF PEASANT FAMILY BY AGE OF MALE HEAD OF HOUSEHOLD

Type of Family	Age of Male Head of Household					Total
	18-27	28-37	38-47	48-57	58-90	
Extended: Parents or Unmarried Siblings	60% (26)	45% (78)	19% (39)	5% (7)	3% (2)	152
Extended: Married Child	0% (0)	0% (0)	1% (2)	4% (5)	12% (7)	14
Nuclear	40% (17)	55% (94)	80% (168)	91% (121)	85% (52)	452
Total	43	172	209	133	61	618

percentage of those with married sons starts to rise, reaching 12 per cent in the final age group. In short, when the head of the household was under 40, in the years when we would expect to find his parents still alive and his brothers and sisters too young to leave home, half of the families were extended. In those households headed by men over 58 a small proportion lived with a married child. But as none of these married sons or daughters had children who were more than two years old, the status of "married son" probably lasted only a few years before the parents retired. Hence the heir would be placed in a younger age category as a householder whose parents lived with him.

The composition of the extended group itself goes through a cycle: the elderly couple may retire and turn over the farm to the eldest son while another child is still living at home. If the census is taken at that point in time, we find a complete stem family. Two years later the child

²³ Thomas K. Burch, "Some Demographic Determinants of Average Household Size: An Analytic Approach," *Demography*, 7 (1970): 61, and "The Size and Structure of Families: A Comparative Analysis of Census Data," *American Sociological Review*, 32 (1967): 347-63.

²⁴ Elsewhere Burch has warned "that the relationship of family structure to age of head is so strong that the age of head (or some other measure of family life cycle stage) ought to be controlled for in any empirical study of typical family forms or of group differences in family structure." Thomas K. Burch, "Comparative Family Structure: A Demographic Approach," *Estadística*, 26 (1968): 291. The possibilities of the method are fully demonstrated by Paul Glick, *American Families* (New York, 1957).

may have left home and the retired father may have died, leaving only a widow. The evidence for the presence of a stem family has vanished, but the stem family organization remains nevertheless. Although from the developmental point of view there can be no such thing as a "true" percentage of stem families in any population, we can gauge the importance of the stem family organization by examining the composition of the extended family groups (see table 2). If there is a large proportion of

TABLE 2. COMPOSITION OF EXTENDED GROUP IN RELATION TO
HEAD OF HOUSEHOLD

<i>Extended Group</i>	<i>Number</i>	<i>% of Extended</i>	<i>% of Total Population</i>
Married Child	14	8%	2%
Retired Couple	57	35	9
Retired Widower	17	10	2
Retired Widow	59	36	9
Unmarried Sibling	19	11	3
Total	166	100%	25%

married children, retired couples, or widowers, there is strong evidence of a stem family structure. In this Austrian case they represent over half of the extended families and thirteen per cent of all households. The presence of a widow or unmarried elderly siblings is ambiguous by itself, but given the strong evidence of the stem family from the census and legal sources it is very likely that they represent the end of the life cycle of a stem family group.

The significance of the total percentage of stem families is further increased if we consider the effects of the high mortality prevailing everywhere in eighteenth-century Europe. The only sure empirical evidence of a stem family is the presence of an elderly couple living in the same household with one married child and his family. But considering the low life expectancy of the period this is an unrealistically stringent criterion. Certainly few children would have had living grandparents. In fact two theoretical calculations of family composition under preindustrial demographic conditions estimate respectively that only 27 or 29 per cent of the families could contain three generations.²⁵ In this light the proportion of stem families found in this Austrian census seems quite high.

There are not only demographic limitations to the formation of stem

²⁵ E. A. Wrigley, *Population and History* (New York, 1969), 131-34; David V. Glass, introduction to *London Inhabitants Within the Walls 1695*, London Record Society (London, 1966), xxxiv, n. 1. Using slightly different assumptions, Wrigley estimates 29 per cent and Glass 27 per cent three-generation families in the population. Furthermore, Marion J. Levy, Jr. has theorized that no society would have more than 50 per cent extended families. Levy, "Aspects of the Analysis of Family Structure," in Ansley J. Coale *et al.*, *Aspects of the Analysis of Family Structure* (Princeton, 1965), 40-63.

families, but also economic limitations, for they can exist only on farms that produce enough income to support three generations. On the large farm the peasant could make a comfortable retirement arrangement with his oldest son and could afford to retire early, so the rich farms were the most likely to include retired parents and also to be owned by very young householders. Of the small number of men who already headed a household before age 28, over half (9 out of 17) owned farms paying the highest taxes. It is not surprising, therefore, to find that there is a distinct difference between the family structure of the rich and the poor: 15 per cent of the poorer as opposed to 34 to 42 per cent of the richer farms showed evidence of a stem family organization (see table 3).

TABLE 3. TYPE OF PEASANT FAMILY BY VALUE OF FARM PROPERTY

Type of Family	Value of Farm Property (in florins) ^a				
	1-49	50-149	150-249	250-349	350+
Extended	15%	15%	34%	35%	42%
Nuclear	85%	85%	66%	65%	58%
Number of Cases	56	27	53	34	36

^a Figures for property value are found in NOLA, Theresianische Fassion 870 (Heidenreichstein). This land survey of 1752 was used as the basis of tax assessments for the rest of the century. The value of the family's property has been determined for Heidenreichstein and seven villages, or about one-third of the peasant households in the census.

If the family owns no land at all the extended family will rarely be found: relatives were living with only 2 out of 109 lodger families and with one out of the 51 families of landless laborers who lived in village-owned houses. This tends to support the view that the small nuclear family has always been more common among the poor and the landless classes as their only practical means of existence.²⁶ Hence the presence of a stem family in any particular household depends both upon the age of the head of the household and the size or value of the farm. The older the householder the less likely that his parents will still be alive; the smaller the farm the less likely that it can support a stem family at all.

Laslett's argument denying the existence of the stem family in England is based largely on aggregate demographic statistics. He finds that only ten per cent of households in one hundred English parishes in his sample were extended families, and he concludes that "the stem family is scarcely in evidence at all."²⁷ I have tried to show that aggregate census data indicates only the proportion of stem families in the extended phase at a given time, but that the significance of this proportion cannot be evaluated without taking into account the age structure of the population and the distribution of wealth.

²⁶ Goode, *World Revolution*, 12-13, 17; Walter, "Kritik," 65-66.

²⁷ "Size and Structure," 218. Laslett restates this in "The Comparative History of Household and Family," *Journal of Social History*, 4 (1970): 77 and n. 3.

On the one hand, Laslett's figures on household composition have not been broken down into age-specific categories although family structure is closely linked to the age of the head of household or some other indicator of the family life cycle. On the other hand, while the literature on stem families has always assumed its link to specific social strata, in particular the landed peasantry, only half of Laslett's sample is made up of agricultural households, and many of these would not fit the category of landed peasants. Since the sample is so heavily weighted against the peasantry, it is not surprising that the proportion of stem families is so low. Laslett's own figures show that laborers are less likely to live in extended families than yeomen (8 per cent as compared with 17 per cent), and Tranter's excellent study of cottager families in Cardington also found only 8 per cent living with relatives.²⁸ A community with few peasants, composed instead of cottagers, agricultural laborers, and other poor classes, is not likely to have a stem family organization, because poverty makes it difficult to form such families. Drake's figures for three villages in Norway in 1801 underline the point: the households of peasant farmers had 26, 34, and 49 per cent extended families compared to 6, 12, and 14 per cent for crofters.²⁹

England was, of course, the first European country to experience a substantial decline in its peasant population, and the process took place during the same period as Laslett's censuses, from the sixteenth to the nineteenth centuries.³⁰ Recent studies have shown that during this same period the village population in many parts of Germany shifted from a majority of landed peasants to a majority of cottagers. The absolute number of peasants remained stable but declined as a proportion of the whole population as the number of cottagers and gardeners multiplied.³¹ A particularly dramatic case was Saxony, where the percentage of cottagers in the population increased sixfold from 5 per cent in 1550 to 30 per cent in 1750, while the proportion of peasants was cut in half, falling from 50 to 25 per cent.³² Given the strong relationship between wealth and family structure one would expect to find a shift in family organization parallel to these changes in society. Specifically, the proportion of stem families in the population should have declined as the proportion of peasants decreased.

So it may be that there was indeed a transition from stem to nuclear families in parts of Western Europe, but not in the sense that the classes

²⁸ "Size and Structure," 220, 222; N. L. Tranter, "Population and Social Structure in a Bedfordshire Parish: The Cardington Listing of Inhabitants, 1782," *Population Studies*, 21 (1967): 269-70.

²⁹ Drake, *Norway*, 116.

³⁰ H. J. Habakkuk ("La disparition du paysan anglais," *Annales: E.S.C.*, 20 [1965]: 649-68) argues that the major decline came between 1660 and 1740. The literature concerning this controversial subject is conveniently summarized in the Economic History Society pamphlet by G. E. Mingay, *Enclosure and the Small Farmer in the Age of the Industrial Revolution* (London, 1968).

³¹ Günther Franz, *Geschichte des deutschen Bauernstandes vom frühen Mittelalter bis zum 19. Jahrhundert* (Stuttgart, 1970), 214-27 and the literature cited there.

³² Based on figures from Karlheinz Blaschke, *Bevölkerungsgeschichte von Sachsen* (Weimar, 1967), as printed in *ibid.*, 220.

who lived in stem families ceased to do so. It is more likely that the absolute number of landed peasants—with whom stem families are associated most strongly—has remained stable or declined, while the size of the landless and poor classes has dramatically increased. Hence the “emergence of the nuclear family” may represent the increase in the proportion of those social classes that have not at any time been associated with a stem family organization. Gregory King’s estimates for 1688 already indicate that less than one quarter of the families of England were freeholders or farmers. Far from showing that peasant stem families were never prevalent in the past, Laslett’s statistics may simply confirm that the shift from a peasant society had already occurred in England by the end of the seventeenth century.³³

ALTHOUGH THEY are often identical in fact, the family and the household must be analyzed separately since the household can also include nonfamily members. On the manor of Heidenreichstein in 1763 servants and lodgers were found in nearly half (46 per cent) of all the peasant households, and their presence can also be explained in terms of the developmental cycle. A servant (*Knecht* or *Dirn*) was someone employed by the family in whose house he dwelt, sharing their food and shelter as part of his wages. A lodger (*Inwohner*) was also a resident, but was not usually a steady employee of the householder and had to pay for his lodging. Lodgers were ordinarily landless laborers, working somewhere in the community, but might at times be hired by the families with which they lived. Servants and lodgers are not always easy to tell apart. The servant was an addition to the labor supply of a particular household; the lodger was a potential supplement to the labor supply of all households in the community.³⁴ Servants were found in about one-third of the households (31 per cent), lodgers in one-fifth (19 per cent), and 5 per cent of the households had both.

Less than 10 per cent of servants were over thirty and there were only two cases of a married *Knecht*. But 95 per cent of the people listed as lodgers were over thirty and 75 per cent lived with a child or a spouse. At first it would seem that we are talking about the same people at different stages in their life cycle: a lodger was an older servant who had married. Why bother with the distinction? In the first place, we will show that the presence of servants in a household was determined by different conditions

³³ King’s figures are printed in Laslett, *World*, 32–33. Eighty-five per cent of Laslett’s censuses are post-1688. See “Size and Structure,” 204–05. In the introduction to Peter Laslett, ed., *Household and Family in the Past* (to be published in 1972), Mr. Laslett replies to some of the points made in this article.

³⁴ Brunner, *Land und Herrschaft*, 316; Feigl, *Grundherrschaft*, 104, 126; Hertha Hon-Firnberg, *Lohnarbeiter und Lohnarbeit im Mittelalter und zur Beginn der Neuzeit* (Baden-Wien, 1935), 8–10, 76–79; Wolf Helmhard von Hohberg, *Georgica curiosa* (Nuremberg, 1716), bk. 1: 72. This is a famous handbook of household economy written by a petty noble of the Waldviertel in 1682 and described in the stimulating book by Otto Brunner, *Adeliges Landleben und europäischer Geist: Leben und Werk Wolf Helmhards von Hohberg, 1612–1688* (Salzburg, 1949).

than was the presence of lodgers, and, second, servants were not really a social class.

Servants should not be viewed as a distinct social class, but as young persons engaged in certain economic functions in another person's household for a limited period of time. They could be either children of lodgers or of landed peasants, and this was what determined a servant's social class.³⁵ For children of peasants, especially girls and nonheirs, a few years as a servant (*in Dienst*) was a normal part of growing up. If there were more children than were necessary to work the farm, some of them would be sent off to work elsewhere until they had earned sufficient money to marry or until they received their inheritance portion.³⁶ For children of lodgers the years spent as servants began earlier, and many of them remained servants or day laborers for the rest of their lives.³⁷

The legal definition of the household was very precise in Austria: it meant all people living in the same house under the authority of the head of the household, whether or not they were members of the family. Moreover, as Otto Brunner has pointed out in his marvelous essay on the concept of the household in European history, the word family was not commonly used in German until the eighteenth century—before that people spoke of belonging to a house or a household.³⁸ The *Universal Lexikon* (1735), for example, defined the family as “a number of persons subject to the power and authority of the head of household either by nature or by law”³⁹—“nature” referred to children, “law” to wife and servants. The idea that the entire household and not only the nuclear family group was the real basis of the peasant family organization was championed in the mid-nineteenth century by the German sociologist Wilhelm Riehl, who, like Le Play, idealized the big happy peasant family. Riehl described the social unit that he felt was rapidly disappearing in the modern world as

the entire household [*das ganze Haus*], which since ancient times includes not only the natural members of the family, but also all those voluntary members and workers called *Ingesinde* [servants]. In *das ganze Haus* the blessing of the family is extended to whole groups of people otherwise without families; they are drawn, as if by adoption, into the moral relationship of authority and piety.⁴⁰

In the peasant household servants were not simply employees, they were part of the family. Even at the beginning of this century a local folklorist

³⁵ Karlheinz Blaschke, “Soziale Gliederung und Entwicklung der sächsischen Landbevölkerung im 16. bis 18. Jahrhundert,” *Zeitschrift für Agrargeschichte und Agrarsoziologie*, 4 (1956): 146.

³⁶ Karl S. Kramer, *Bauern und Bürger im nachmittelalterlichen Unterfranken* (Würzburg, 1957), 155. In the marriage records of the parish in Heidenreichstein there are many examples of girls *in Dienst* in the town returning to their village to marry.

³⁷ Feigl, *Grundherrschaft*, 24, 130.

³⁸ Otto Brunner, “Das ‘ganze Haus’ und die alteuropäische ‘Ökonomik’” in his *Neue Wege der Verfassungs- und Sozialgeschichte* (2d ed.; Göttingen, 1968), 108, 111.

³⁹ “Familia,” in *Grosses Vollständiges Universal Lexikon* (Halle, 1732–50), 9: col. 205.

⁴⁰ Wilhelm Riehl, *Die Familie* (Berlin, 1854), 147.

of the Waldviertel noted that servants were counted as part of the family and usually ate with the family out of the same pot.⁴¹ For Riehl this was a crucial fact, for

in many peasant villages a single circumstance—whether or not the entire household, including the servants, eat at the same table—is sufficient to settle whether the relation of master and servant has become a purely legal affair or whether it is still somewhat patriarchal; whether old customs have disappeared entirely or whether they have been maintained and developed.⁴²

We have no way of knowing what the normal relationship between servants and masters was in the eighteenth century, but there is considerable evidence that it has never been as harmonious as romantics like Riehl would have us believe.⁴³ Certainly the oral agreement to work for a peasant was more than morally binding, for it was considered a legal contract and usually ran for only one year at a time. The local village laws were full of restrictions and regulations concerning servants. They could not leave before one full year was up, nor could they be dismissed without good reason. A common regulation prevented peasants from stealing servants from one another by offering higher wages. The lack of trustworthiness credited to servants is indicated by the laws that forbade the purchase of anything from a servant until the buyer checked with the master to see that it was not stolen.⁴⁴

Some of the personal antagonisms of the peasant-servant relationship are expressed in the “Knechtslied,” a widespread Austrian folksong first written down in the early nineteenth century. The first part of the song lists the peasant’s complaints: servants are too expensive, never work hard enough for their pay, and spend their time chasing after girls and the peasants’ own wives. In the Waldviertel only the second part of the song, in which the servant answers, was common.⁴⁵

Harr narischa Baua
Was fällt da denn ein
Und wann dir die Knecht ztheua san
So stellst du Koan ein.

You stupid old peasant,
Your mind is half gone.
If a servant’s too costly,
Then don’t take one on.

⁴¹ Rauscher, “Volkskunde,” 22.

⁴² Riehl, *Die Familie*, 150. This translation is from Pitrim Sorokin et al., *A Systematic Sourcebook in Rural Sociology* (Minneapolis, 1931), 2: 95.

⁴³ All of this must be considered in the context of an older sociology that was concerned with the decline of *Gemeinschaft* and with the replacement of personal relationships by contractual ones. But the myth of the happy life on the family farm is hard to kill and long continued to be a mainstay of conservative sociologists such as Hans Günther, *Das Bauerntum als Lebens- und Gemeinschaftsform* (Leipzig, 1939), 191–93. The harsher realities are described in J. Walleitner, *Der Knecht* (Salzburg, 1947).

⁴⁴ Feigl, *Grundherrschaft*, 126–27; Rauscher, “Geschichte des bauerlichen Wirtschaftsleben,” in Stepan, *Das Waldviertel*, 7 (Vienna, 1937): 139–41. Kaiserlichen Akademie der Wissenschaften, *Österreichische Weistümer* (Vienna, 1870–1913), vol. 8, a collection of village laws in the Waldviertel, shows evidence of these restrictions on nearly every page.

⁴⁵ Hermann Strobach, *Bauernklagen: Untersuchungen zum sozialkritischen deutschen Volkslied* (Berlin, 1964), 182–87 gives the history and various versions of the “Knechtslied.”

Baua du hast es
Nöt guat in dein Lei?
Wann ma aussì in d'Arbeit gehn,
Bleibst bei dein Wei.

Und wann i in's Gasseln geh,
Eiferst du glei,
Und i geh zu mein Diarndl
Und nôt zu dein Wei.

Peasant, don't you have
A rich enough life?
While I go out to work,
You stay home with your wife.

And when I go courting,
Your jealousy's rife,
But I go to my sweetheart
And not to your wife.

Landless persons had a difficult time living in a society whose membership was defined in terms of landownership. They were not allowed to marry or settle in a village without community approval. When they lost positions they often had little recourse other than begging until they found other jobs. In fact the authorities saw little difference between vagabonds and unemployed servants, and the poor laws of the eighteenth century were mostly concerned with rounding up the "roaming beggars and other riffraff without masters" in order to send them back to the village of their birth.⁴⁶ Old Johann Hechenberber, for example, had worked for 60 years in villages near Heidenreichstein, as a servant for 19 years in the town of Vitis, 26 years in Rupprechts, 4 years in Gopprechts, 9 years in Klein Poppen, and one year in Seyfritz and as a cowherd for one year in Dietweis. After all these years of service he was unable to find work in his old age and was arrested for begging.⁴⁷

The servants' main function in the peasant household was as a labor substitute for children. This means that servants were more likely to be found in households with no or few children or in households headed by a young couple whose children were not yet old enough to work. In the villages of the Waldviertel in 1763 the number of servants in a household was negatively related to the number of children in the peasant's family (Pearsonian correlation coefficient, $r = -.17$),⁴⁸ the age of the oldest

⁴⁶ Feigl, *Grundherrschaft*, 84, 128-29; Hon-Firnberg, *Lohnarbeiter*, 79; August Rothbauer, "Bettlerunwesen im 17. Jahrhundert," *Das Waldviertel*, n.s. 8 (1959): 112-15.

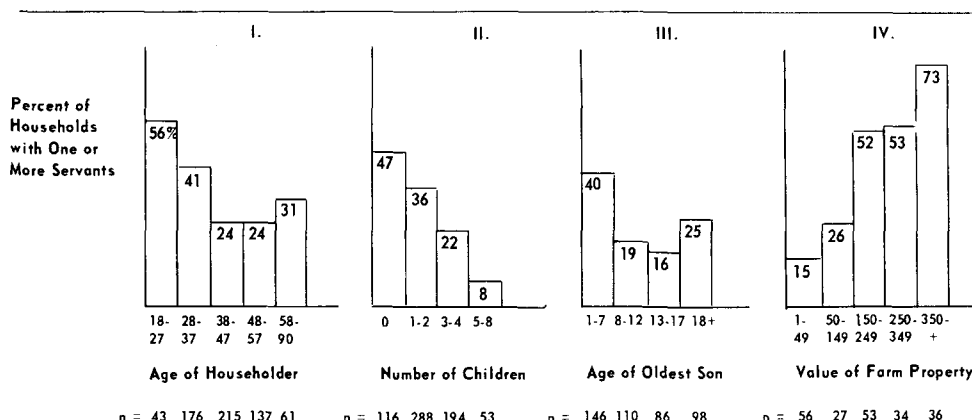
⁴⁷ Schlossarchiv Heidenreichstein 3/131 (Hauptvisitation der vagierende Gesindl 1724), which contains a number of interrogations of vagabonds.

⁴⁸ The Pearsonian correlation coefficient (r) measures the direction and strength of a linear (straight line) relationship between two variables. It has a range of -1.0 to $+1.0$. Inverse relationships have a negative sign, and direct relationships have a positive sign. The closer the coefficient comes to -1 or $+1$, the stronger the correlation; the closer it comes to zero, the weaker the correlation. The significance level chosen (here .05) establishes which coefficients will be rejected as statistically not significant (ns).

CORRELATION MATRIX OF VARIABLE PAIRS ($n = 220$)

Variables:	Value	Age	Children	Kin	Servants
Prop. Value	+1.00				
Age of					
Householder	-.07ns	+1.00			
No. of Children	+.11ns	+.31	+1.00		
No. of Kin	+.18	-.34	-.16	+1.00	
No. of Servants	+.56	-.19	-.17	+.25	+1.00
No. of Lodgers	-.09ns	+.05ns	+.08ns	-.13	-.16

TABLE 4. PRESENCE OF SERVANTS IN HOUSEHOLD BY I. AGE OF HOUSEHOLDER; II. NUMBER OF CHILDREN IN HOUSEHOLDER'S FAMILY; III. AGE OF HOUSEHOLDER'S OLDEST SON; IV. VALUE OF FARM PROPERTY (IN FLORINS)



son ($r = -.16$), and the age of the head of the household ($r = -.19$) (see table 4).

The Russian economist Chayanov pointed out the importance of the family life cycle in explaining the economic behavior of peasants. He showed that during the first half of a peasant family's life cycle the economic well-being of the family declines as each new child adds to consumption but not to the output of the farm. During the second half of a family's history, as the children one after another reach working age, there is a proportional rise in economic well-being. In Russia, where the land was constantly being transferred and redistributed, the amount of land farmed by a peasant family rose and fell with the phases of the life cycle.⁴⁹

In Austria the peasant holding was impartible and was transferred en bloc from generation to generation. The amount of land belonging to a family remained relatively constant,⁵⁰ but the amount of labor provided by the family went through the same cycle described by Chayanov, creating a labor shortage in the first years of the family's existence. The obvious solution was to expand the labor force by hiring servants in the early years of marriage to replace the labor of retired parents and the brothers and sisters who had left the farm and to do the work for which the children were still too young. As the children grew up they replaced the servants and even created a labor surplus on the small farms, which is why many of them became servants in other households in their teens. In Russia, where

⁴⁹ A. V. Chayanov, *The Theory of the Peasant Economy*, ed. Daniel Thorner et al., (Homewood, 1966), 55-69.

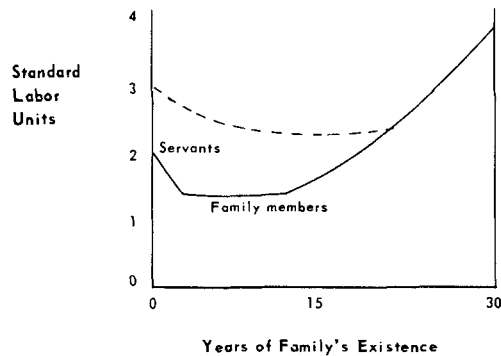
⁵⁰ There were actually two kinds of land, the impartible peasant land (*behaustes Gut*) and the partible *Überland*. The latter represented 18 per cent of the land in the manor, but half of the *Überland* was located in the town itself and owned by small holders. In the villages the sale of *Überland* allowed peasants to increase their holdings slightly, but the bulk of the peasants' wealth lay in the impartible holding. NOLA, Theresianische Fassion 870 (*Überland*); Feigl, *Grundherrschaft*, 59-62.

few peasant families kept servants, stability was achieved by expanding or contracting the size of the farm. In Austria, where the land was impartible, it was achieved by hiring or dismissing servants.⁵¹

The relationship of the Austrian peasant family's life cycle to the amount of labor available in the household can be shown by estimating the amount of standard labor units contributed by each family member. Householders and grown servants would add a full labor unit, a wife added somewhat less, especially during the childbearing years, and children added increasing fractions as they grew up.⁵² Table 5 depicts a hypothetical cycle to show the

TABLE 5. LABOR UNITS AVAILABLE IN HOUSEHOLD AT DIFFERENT PHASES OF FAMILY LIFE CYCLE

Years of Family's Existence	Standard Labor Units Contributed					Total Servants	Total Household
	Peasant Householder	Wife	Son	Daughter	Total Family		
0 Marriage	1	.8	—	—	1.8	1	2.8
1-12 Years of Childbearing	1	.4	—	—	1.4	1	2.4
12-20 Young Children	1	.8	.5	.5	2.8	—	2.8
20-30 Older Children	1	.8	1.0	.8	3.6	—	3.6



Adapted from S.H. Franklin, *The European Peasantry: The Final Phase* (London, 1969), 19.

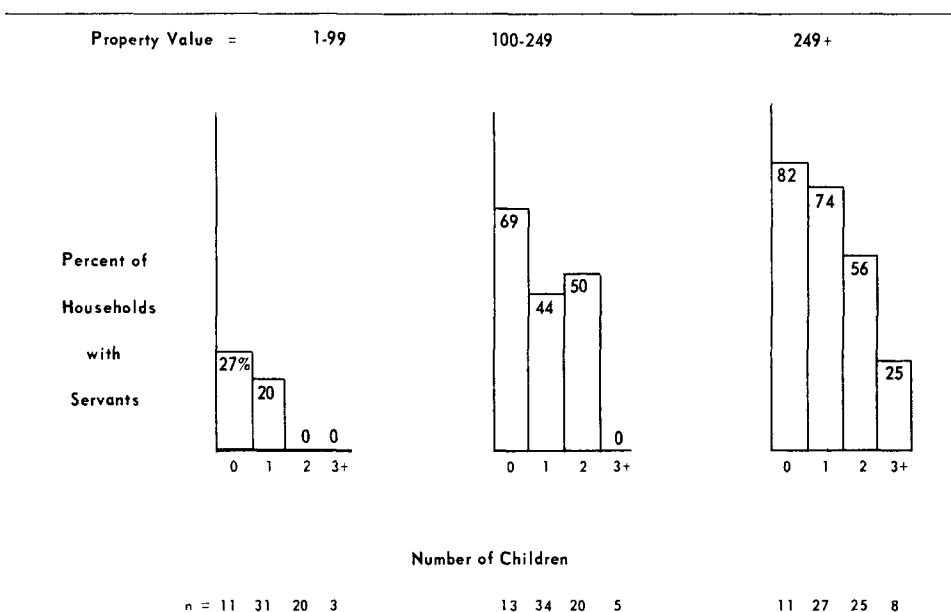
importance of servants in maintaining the stability of the household labor supply.

The presence of servants in a household was positively related to the value of the property ($r = +.56$). The richer peasants were the most likely to need servants and could most afford to hire them (see table 4: IV). Within each economic group, the inverse relationship between servants and children still holds, but there is a steady increment as the property value increases. Servants were not kept by poor peasants if they had more than one child or by middling peasants if they had more than two children,

⁵¹ See Chayanov, *Peasant Economy*, 110-12.

⁵² S. H. Franklin, *The European Peasantry: The Final Phase* (London, 1969), 18-20.

TABLE 6. PRESENCE OF SERVANTS IN HOUSEHOLD BY NUMBER OF CHILDREN IN HOUSEHOLDER'S FAMILY, CONTROLLING FOR VALUE OF PROPERTY (IN FLORINS)



and rich peasants were least likely to keep a servant if they had three or more children (see table 6). Servants were a labor substitute for children within each economic group, but the labor requirements of the farm obviously increased proportionally to its value and size.

One-fifth of peasant households included a lodger. Ordinarily there was only one lodger or lodger family per household, but fourteen households had two, and one household even had three. Three-quarters of the lodgers lived with a spouse or a child: over half were married couples, about one-quarter consisted of single parents with children, and another quarter were single individuals, almost all of them women. While the unmarried servant clearly functions as a labor substitute for children in the household, the economic role of the lodger is not so clear, because we do not know whether any particular lodger exclusively or even primarily worked for the peasant in whose household he lived. The lodger could fulfill two opposite economic needs for the peasant: rentpayer or laborer. As someone who paid rent but did not work for the peasant in whose house he resided, he was a source of income for the household and served as a complement to the labor supply of the whole community, providing extra help wherever it was needed in the village. But sometimes the lodger was really an employee of the household where he resided and had the same function as any servant.⁵³ Because the lodger could be either a profit (as a rentpayer) or a cost (as a paid worker) to the household, there is no clear linear relationship between the presence of lodgers and the number of

⁵³ Hon-Firnberg, *Lohnarbeiter*, 77-78; Feigl, *Grundherrschaft*, 128, 131.

children in a family, the age of the head of the household, the age of the oldest son, or the value of the property. (No correlations are significant.)

The only significant indicator for the presence of a lodger in a household is the absence of an extended family ($r = -.13$). It would seem that the main reason for a lodger's being found in a household was that there was a room available to house one. A lodger could be taken in when the parents died and the *Stübl* was empty. Lodgers were half as likely to be found in households of extended families (10 per cent) as in those of nuclear families (23 per cent). The reverse is true for servants, who were half as likely to be found in nuclear families (26 per cent) as in extended families (47 per cent). Householders with retired parents needed servants because parents did not continue to participate fully in farmwork once they had retired. And servants, who slept with the children, did not take up a room in the house.

The fixed space available for lodging, the fixed farm size that requires a constant labor force, and particularly the stem family organization explain why the mean household size was so stable throughout the developmental cycle of the household. Grandparents die as children are born; servants leave as children grow older; the heir remains to begin his own family while his parents retire and his siblings depart. As a result the mean household size varies only from a high of 5.76 to a low of 5.42 persons over the entire household cycle (see table 7). This stability in size throughout the

TABLE 7. MEAN HOUSEHOLD SIZE BY AGE OF MALE HEAD OF HOUSEHOLD

Household	Age of Male Head of Household					All
	18-27	28-37	38-47	48-57	58-90	
Mean Household Size						
(Number of Persons)	5.65	5.68	5.67	5.76	5.42	5.63
Number of Households	43	176	215	137	61	651

NOTE: Discrepancies in the totals in tables 1, 2, and 7 result from the fact that there were 19 households headed by females and 14 households whose family composition could not be determined. Table 2 includes both groups ($n = 651$), table 7 excludes female heads ($n = 632$), and table 1 excludes both ($n = 618$).

developmental cycle of the household is probably a distinguishing feature of a stem family structure, since we would expect nuclear family households to begin small, reach their peak size when the parents are in their forties and all the children have been born, and then decline in size again as the children leave home.⁵⁴ Even using this developmental approach, mean

⁵⁴ My calculations of mean household size for three villages in the Norman Pays de Caux in 1793, where there was no peasant stem family structure, provide the following comparison (from Archives Départementales de la Seine-Maritime, L 1599):

Age:	20-29	30-39	40-49	50-59	60-
Size:	3.1	4.2	4.8	4.5	3.6

In a study of five communities Laslett has found a similar pattern in England, but with a peak of over six persons in the households headed by persons 40 to 49. Private correspondence.

household size has limited usefulness as a measure of household or family structure.⁵⁵ The mean household size was nearly the same at every stage of the life cycle in these Austrian villages, but the underlying social composition of the household varied considerably.

The social structure of this Austrian peasant society has been described in terms of the three parallel developmental cycles of the individual, the family, and the household. The phases of the *individual's* cycle are: son and heir; head of household; retired parent. The critical events in his life cycle are his marriage and the transfer of authority through retirement or inheritance. The *family* cycle is formed by the concurrence of the individual cycles of successive generations that constitute the stem family. The phases of the stem family alternate between extended and nuclear: the parents retire and the heir marries; the parents die and the heir heads a nuclear family; the heir retires and his son marries. The determinants of the phases are demographic events—marriages, births, and deaths. The developmental cycle of the *household* is dependent upon the phases of the family cycle and the presence or absence of nonfamily members. The typical phases are: an extended family with servants and young children; a nuclear family without servants and with older children; a nuclear family with lodgers or an extended family without them. The determinants are the economic need to maintain a stable labor force on the farm and the availability of living space.

As the composition of the household changed, personal relationships within it were affected. Only the head of the household had full legal rights, and with them came complete authority over all the other members. The children in the household were thus legally and financially dependent upon the father until, in a contract, he arranged for their marriage or succession. When land is the main or only source of wealth and authority, and when the amount of land is limited, there are sure to be struggles for its control: struggles among children to become heirs, between parents and children over the transfer of authority. When farm labor is relatively scarce, required only seasonally, and paid for from the earnings of peasants with marginal incomes, there are sure to be conflicts between peasants and servants. These social strains are reflected in laws limiting servant wages to the going rate, forbidding anyone from stealing his neighbor's servants, and forbidding servants from leaving a farm without permission. Dissatisfaction shows up in folk songs in which the farmer complains of his servant's laziness, disobedience, and immorality and is answered by the servant's song complaining of the peasant's enjoyment of a comfortable life at his expense. The peasant's entire household may have looked like a big happy family, but it was held together by legal restrictions imposed in the peasant community and by the limited opportunities offered by the rural economy.

⁵⁵ Burch concludes that mean household size reflects fertility mainly and has little to do with extended family structure. "Size and Structure," 363.

Toward a Psychoanalytic Interpretation of Bismarck

OTTO PFLANZE

THE INCREASING INTEREST of psychoanalysts in history and of historians in psychoanalysis stems from a mutual need. The former have found it necessary to give greater attention to social and historical influences on the development of personality, while the latter are seeking a better understanding of irrational and unconscious influences on individual and group behavior in history.¹ On both sides the difficulties are formidable, most psychoanalysts knowing too little history and most historians too little psychoanalysis. Biographers have a special handicap: the subject is dead or, if alive, usually inaccessible to analysis. Material for analysis may be found in his writings, in his recorded speeches and conversations, and in the observations of others; yet this evidence will never suffice, for the primal experiences that produce the basic character traits occur in the earliest years of life and are hidden from the subject himself by repression and other mechanisms of defense. Often they can be uncovered only after months and years of careful work by the subject interacting with a skilled analyst.

Another obstacle for the historian is skepticism²—a tendency to regard

I express my appreciation to the University of Minnesota and the Institute for Advanced Study at Princeton for grants that made possible the research and writing of this article. Fred Weinstein of the State University of New York at Stony Brook was extremely helpful in providing references to pertinent psychoanalytical literature and in making suggestions for the revision of the manuscript. I am also indebted to Carl Schorske of Princeton University, Felix Gilbert of the Institute for Advanced Study, and David Kieft of the University of Minnesota for important improvements in the text. In different form the essay was given as a lecture at Rutgers University in February 1971 and at the State University of New York at Albany in October 1971.

¹For a survey of the growing literature on the subject see Hans-Ulrich Wehler, "Zum Verhältnis von Geschichtswissenschaft und Psychoanalyse," *Historische Zeitschrift*, 208 (1969): 529-54, particularly the literature cited on pp. 540-41. See also Benjamin Wolman, ed., *The Psychoanalytic Interpretation of History* (New York, 1971), and Bruce Mazlish, ed., *Psychoanalysis and History* (2d ed.; New York, 1971).

²Most recently expressed by Jacques Barzun, "History: The Muse and Her Doctors," *AHR*, 77 (1972): 36-64. No doubt Barzun will soon receive the rejoinders he obviously expects. I find myself somewhere between the two camps, the most vulnerable position on any battleground. Certainly Barzun has exposed, with his usual wit and urbanity, many of the hazards in using "tools" provided by psychology and other social sciences. Yet historians, being engaged in a risky enterprise at best, ought not to refuse any help they can get. As Barzun admits in his more generous moments, not all the help offered is illusory. History refracts into many colors and by now everyone knows, or ought to know, not to expect a pot of gold at the end of the rainbow.

Freudian psychology, as Frederick the Great did Leibniz's monadology—"a novel by a man of genius." Indeed there are grounds for caution, both because of the complexities of the field and because of its highly speculative character. Historians are accustomed to demand more terra firma than psychoanalysts can usually offer; yet historians are becoming increasingly bold in their search for new ways to comprehend the past. "In humane studies there are times when a new error is more life-giving than an old truth, a fertile error than a sterile accuracy."³ Historical writing, like psychoanalysis, is marked more by its failures than its successes; the failures, because of the questions they raise, are often the precondition of the successes. Whatever the risks, the biographer at least can no longer ignore the import of psychoanalysis for his work. Let those who think otherwise reflect on Erikson's words:

It is necessary to contemplate (if only as a warning to ourselves) the degree to which in the biography of a great man "objective study" and "historical accuracy" can be used to support almost any total image necessitated by the biographer's personality and professed calling; and to point out that biographers categorically opposed to systematic psychological interpretation permit themselves the most extensive psychologizing—which they can afford to believe is common sense only because they disclaim a defined psychological viewpoint. Yet there is always an implicit psychology behind the explicit antipsychology.⁴

The nature of psychoanalytic literature offers some help for the historian, particularly for biographers. It abounds in models based on clinical experience and in broad generalizations about character types and typical psychic problems, which can be used to illuminate specific personalities. Like suits, they can be tried on for size and shape and sometimes tailored to fit. This is what I, having had no special training in psychoanalysis, attempted in this study. The results are presented as speculative, not definitive.

OTTO VON BISMARCK's dominant characteristic was his will to power, the unremitting drive to master men and events that made him the dominant statesman of his time not only in Germany but in Europe. He himself provided the best testimony of this characteristic in a famous letter of 1838 in which he wrote,

My ambition strives more to command than to obey—these are facts for which I can give no explanation other than my personal taste. . . . Few famous statesmen, especially in countries with absolutistic constitutions, have been motivated by patriotism to enter the state service; much more often the motives have been

³ H. R. Trevor-Roper, *History, Professional and Lay, An Inaugural Lecture Delivered before the University of Oxford on 12 November 1957* (Oxford, 1957), 22.

⁴ Erik H. Erikson, *Young Man Luther: A Study in Psychoanalysis and History* (New York, 1962), 35–36.

ambition, the wish to command, to be admired, and to become famous. I must confess that I am not free from this passion.⁵

That this self-appraisal was no idle fancy is shown by characteristic episodes from his early career: his provocative beginning as a student at Göttingen when he challenged most of a student corps; his abandonment of a career in the Prussian civil service on the grounds that he could not follow the commands of others "like a musician in an orchestra"; his first appearance as a deputy in the United Diet of 1847 when he provoked an uproar with outrageous statements; his attempt to mobilize an immediate counterrevolution in March 1848; his debut as a diplomat in Frankfurt in 1851 when he immediately challenged the leadership of the Austrian delegate who presided over the Federal Diet; and his persistent attempts, through a steady barrage of letters and dispatches, to establish his influence over both the foreign and domestic policy of the government in Berlin from 1851 to 1862.

Within nine years after he gained power in 1862 he changed the face of Germany and Europe. After 1871 he considered Germany to be a satiated power, and hence his foreign policy during the last two decades was essentially unaggressive (except for the acquisition of colonies in the 1880s). His quest for power found its satisfaction in those years in domestic affairs: the attacks upon the Center and Social Democratic parties, his manipulation of the conservative and liberal parties, his persistent and successful effort to remove from office independently minded ministers of the Prussian cabinet, his construction under his direct authority as chancellor of a Reich executive that overshadowed the Prussian cabinet, and his accumulation of offices (chancellor, minister president, foreign minister, and minister of commerce). The steady ascent of his personal authority in these years led contemporaries and historians to speak of a "Bismarck dictatorship."⁶ Despite numerous threats of resignation over the years, he clung tenaciously to power in 1890 and never forgave the emperor who dismissed him or the "intriguers" who persuaded him to do it.

The one period in his life when this drive for power appears somewhat abated—from 1838, when he quit the civil service, to 1847, when he commenced his political career—needs special attention. During this period, from age twenty-three to thirty-two, he managed family estates in rural Pomerania and Brandenburg, assisted his brother as Landrat, and acted as dyke reeve on the Elbe. While he was successful at these activities, the life itself did not satisfy him for long. He drank, gambled, hunted, traveled to Switzerland, France, and Britain, and made vague plans for a trip to the

⁵ Bismarck to his father, Ferdinand von Bismarck, Sept. 29, 1838, in Otto von Bismarck, *Die gesammelten Werke*, ed. Herman von Petersdorff et al. (Berlin, 1923-33), 14: 14-15.

⁶ See Heinrich Heffter, "Die Kanzlerdiktatur Bismarcks," *Abhandlungen der Braunschweigischen Wissenschaftlichen Gesellschaft*, 14 (1962): 73-90.

Middle East and India. This period of gestation corresponds to the "psychosocial moratorium" that Erikson has found in several highly creative personalities: Martin Luther, Sigmund Freud, William James, George Bernard Shaw, and, one suspects, Erikson himself.⁷ In the restless, unhappy Junker one can see many of the typical characteristics of the identity crisis of young adulthood. While never questioning the rightness of the social order or of his own privileged status as an aristocrat, he was contemptuous of the neighboring gentry, made fun of their ignorance and piety, argued with them about politics and religion, and delighted in shocking them by boorish behavior—which earned him locally the sobriquet of "crazy Bismarck." During these years he also, typically, formed a deep attachment for a sibling, his younger sister Malwine, who kept house for him at Kniephof until her marriage in 1844.⁸

During the years 1846 and 1847 Bismarck's moratorium was brought to an end by three events that occurred in quick succession—religious conversion, marriage, and the beginning of his political career. The fact that Johanna von Puttkamer was a pious woman, whose pietistic father would never have given her to a nonbeliever, has raised some doubts about the depth of Bismarck's religious experience. Yet the evidence of its genuineness is overwhelming and there is no good reason to doubt that he lost his skepticism (he spoke variously of deism, atheism, pantheism) in favor of pietistic Lutheranism.⁹ Marriage provided him with a secure base from which to act—a stable, dependable, nurturant family life in which he was unquestionably the dominant figure, free from contradiction and from the threat of repudiation or rejection. In parliamentary and diplomatic activity he found the arena in which he could realize himself through the exercise of political power. Erikson's description of what is necessary in "identity formation" fits Bismarck almost as well as it did, in this case, Shaw.

Man, to take his place in society, must acquire a "conflict free," habitual use of a dominant faculty, to be elaborated in an occupation; a limitless resource, a feedback, as it were, from the immediate exercise of this occupation, from the companionship it provides, and from its tradition; and finally, an intelligible theory of the processes of life which the old atheist, eager to shock to the last, calls a religion.¹⁰

Bismarck, it is often asserted, did not covet power for its own sake, and, if he did not seek the attainment of ideal ends through its use, he did not use it irresponsibly. On the contrary, his religion, his aristocratic background, his traditional view of the art of statecraft imposed restraints. He belonged to the school of classical politics that believed foreign and military policy

⁷ Erik H. Erikson, *Identity: Youth and Crisis* (New York, 1968), 142–61.

⁸ Erich Marcks, *Bismarck, Eine Biographie*, vol. 1: *Bismarcks Jugend, 1815–1848* (6th ed.; Stuttgart, 1909), 200–84; Werner Richter, *Bismarck* (Frankfurt, 1962), 29–39.

⁹ See my *Bismarck and the Development of Germany: The Period of Unification, 1815–1871* (Princeton, 1963), 52–55.

¹⁰ Erikson, *Identity*, 150.

must be dictated by the reasoned interest of state and pursued within the limits of the balance of power system. This "ethic" elevated him above the temptation of personal aggrandizement and shielded him from the passions of ideology and from the irrational influence of popular movements.¹¹ Indeed, Bismarck never again spoke or wrote, so far as the record shows, so frankly of his "wish to command, to be admired, and to become famous" as he did in the letter of 1838. During his political career he saw himself as the "servant of the king," "the man of the state and the king," who stood above the chaos of social and political life, seeking without fear or favor, prejudice or partisanship, the ideal line in foreign and domestic policy dictated by *raison d'état*.¹² "I am," he told the Prussian chamber of representatives in 1874, "a disciplined statesman who subordinates himself to the total needs and requirements of the state in the interest of peace and the welfare of my country."¹³

One need not question the sincerity of this statement in order to establish that the Bismarck of 1874 was still the Bismarck of 1838. What occurred may be understood in terms of a common psychic process that has been called the "evocation of a proxy." Instinctual impulses that the conscience cannot tolerate are either repressed (i.e., "thrust back into the id") or projected (i.e., "displaced into the outside world").¹⁴ Bismarck projected his quest for power and renown onto the Prussian state. Goals that would have been intolerable if conceived as personal, could, when conceived as in the interest of state or public welfare, be sought without a sense of guilt and with all the formidable talents he possessed. What would have been selfish and subjective became altruistic and objective, therefore acceptable to his conscience.

God was the one higher authority to which Bismarck found he could subject himself. What he could accept from no other man, not even the king, he accepted from God. Pietism appealed to him because of its undogmatic character, its reduction of religion to the unmediated relationship between man and God. He had no interest in theology, and, while daily prayer and Bible reading became a habit, he seldom attended church. Religion also supplied the moral strength for victory in what must have been a titanic struggle for self-discipline. In July 1851, at a moment of great

¹¹ See particularly Gerhard Ritter, *Staatskunst und Kriegshandwerk* (Munich, 1954), vol. 1; Hans Rothfels, *Bismarck, Der Osten und Das Reich* (Stuttgart, 1960); Franz Schnabel, "Bismarck und die klassische Diplomatie," *Aussenpolitik*, 3 (1952): 635-42; and Joachim von Muralet, *Bismarcks Verantwortlichkeit* (Göttingen, 1935).

¹² Bismarck, *Die politischen Reden des Fürsten Bismarck*, ed. Horst Kohl (Stuttgart, 1892-94), 6: 129-31; 8: 328-29; 11: 292; 12: 85; Bismarck, *Werke*, 6c: 383.

¹³ Bismarck, *Reden*, 6: 131.

¹⁴ Anna Freud, *The Ego and the Mechanisms of Defence*, tr. Cecil Baines (New York, 1946), 132-46. While Anna Freud described the process, Martin Wangh provided the best descriptive term. The term "altruistic surrender" coined by Edward Bibring and used by Anna Freud has, as Wangh pointed out, a "passive renunciatory" quality that is not always appropriate. He prefers "evocation of a proxy" because of its "active quality, equally unconscious." See his "The 'Evocation of a Proxy': A Psychological Maneuver, Its Use as a Defense, Its Purposes and Genesis," *Psychoanalytic Study of the Child*, 17 (1962): 451-69.

inner stress, Bismarck, who had been compelled to live for many weeks apart from his wife, wrote to a friend, Hans von Kleist Retzow, of the temptations that still pursued him.

The chief weapon with which evil assaults me is not desire for external glory, but a brutal sensuality, which leads me so close to the greatest sins that I doubt at times that I will gain access to God's mercy. At any rate I am certain that the seed of God's word has not found fertile ground in my heart, laid waste as it was from youth. Otherwise I could not be so much the plaything of temptation, which even invades my moments of prayer. Whenever I am alone and unoccupied I have to struggle against visions of an abyss that come from a depraved fantasy that leaps with astonishing agility from the consoling image of him who suffered for our sins to new sinful thoughts. . . . I am often in hopeless anxiety over the fruitlessness of my prayer. Comfort me, Hans, but burn this without speaking of it to anyone.¹⁵

Ultimately he concluded that the "usefulness of prayer" lay in its implied "submission to a stronger power," not in its efficacy for God's intercession. "I am conscious of that Power, which is neither arbitrary or capricious."¹⁶

Here likewise the evocation of a proxy can be seen. After his conversion the quest for power was no longer personal. "I am God's soldier and must go whither he sends me," he wrote to his wife in 1851, "and I believe that he sends me and shapes my life as he requires." Years later, after Bismarck's great successes in foreign affairs, he explained that he had merely listened for God's footsteps echoing through world history and had seized a corner of the divine cloak. The statesman who unified Germany and changed the face of Europe always maintained that man cannot direct the stream of time, but merely steer upon its current.¹⁷ In this way Bismarck protected his ego from the sting of conscience.

THE PRINCIPAL QUESTION in any biography is, of course, how the subject became the sort of person he was. In seeking to explain Bismarck's character and his genius, biographers have indeed, to return to Erikson's comment, permitted themselves the most extensive kind of psychologizing under the guise of common sense. They have been intrigued by the fact that he was the product of two different heritages. His father was a Brandenburg Junker whose family had served for generations in the Hohenzollern armies, while his mother, Wilhelmine Mencken, stemmed from a burgher family of lower Saxony that had produced university professors and civil servants. His maternal grandfather was Anastasius Ludwig Mencken, state

¹⁵ Bismarck to Hans von Kleist Retzow, July 4, 1851, in Muralt, *Bismarcks Verantwortlichkeit*, 89-90. The passage was suppressed by the editors of Bismarck's collected works.

¹⁶ Quoted in Sir W. Richmond, "Bismarck at Home: Personal Impressions," *Daily News* (London), Aug. 2, 1898, p. 5.

¹⁷ Bismarck to Johanna von Bismarck, May 3, 1851, in Bismarck, *Werke*, 14: 208; Arnold Oskar Meyer, *Bismarcks Glaube* (4th ed.; Munich, 1936), 7, 64-65; Muralt, *Bismarcks Verantwortlichkeit*, 63-64.

secretary under Frederick the Great, Frederick William II, and Frederick William III.¹⁸ To Bismarck's biographers it has appeared self-evident that his personal characteristics were acquired by inheritance from these two ancestral lines; the biographers have differed only over whether one or both lines provided him with the genetic heritage that formed his character. Selections from two prominent biographies will suffice to prove the point—the first by A. O. Meyer, a conservative, the second by Erich Eyck, a liberal.

The greater the difference between father and mother and between their ancestral lines, so much the greater is the range of the child's legacy in spirit and character. A fortunate union of opposites produces the birth of a genius. Thus it was in the case of Goethe, Frederick the Great, and Bismarck. Yet nature only succeeds fully in its grand design, if it gives to the genius at the same time the strength of will that will make him master over the two souls in his breast. To have to struggle mightily with themselves is the fate allotted to all the great ones of mankind in the cradle. Only if they win this struggle without being innerly shattered by it will their creative power be free.

Children are certainly not mathematical equations and the attempts to explain the individuality of a genius from characteristics inherited from ancestors who were themselves not highly gifted is particularly hopeless. But even when aware of this limitation one still cannot resist pondering the contribution of the parents and their predecessors. With reservations one can say that the superiority of mind and spirit that distinguished Bismarck from all his contemporaries, and especially from those of his own social class, was inherited more from the mother than from the father.¹⁹

While conservative, Meyer was also bourgeois; hence his solution was dualistic. Eyck was more cautious, first eschewing the genetic interpretation, then yielding to it in the absence of anything better. Being bourgeois and liberal, he naturally chose the Mencken line as predominant, aristocrats not being noted for "superiority of mind and spirit." German intellectuals have found consolation in the thought that, while their predecessors failed to unify Germany in 1848, the man who accomplished it in 1871 belonged by heritage to the aristocracy of the mind as well as that of the soil.

Obviously the traditional explanations of the formation of Bismarck's character will not do. The cultural background of the parents is, of course, significant because of its impact on the mind of the child and young adult, but the inheritance of acquired characteristics has no standing as a scientific theory. For historians to apply it demonstrates the size of the gap between the humanities and the sciences. The gap is also evident in that nei-

¹⁸ Another descendant was the American literary critic H. L. Mencken.

¹⁹ Arnold Oskar Meyer, *Bismarck: Der Mensch und der Staatsmann* (Stuttgart, 1949), 11; Erich Eyck, *Bismarck* (Zurich, 1941-44), 1: 15. Although the literature on Bismarck is enormous—6,138 items in a recent count—the attempts at a psychological interpretation are few, and none approach the problem from the standpoint of psychoanalysis. The most interesting is Karl Groos, *Bismarck im eigenen Urteil, Psychologische Studien* (Stuttgart, 1920). Henry Lowenfeld, "Freud's Moses and Bismarck," *Psychoanalysis and the Social Sciences*, 2 (1950): 277-90, is of no value.

ther Meyer nor Eyck turned to that discipline with the most to offer in explaining character development—psychoanalysis. It is true that psychoanalysis has been more concerned with neuroses and therapy than with character formation, and yet some important work has been done. In a pioneering study, *Character Analysis* (1933), Wilhelm Reich describes a number of personality types, one of which, the “phallic-narcissistic character,” bears some resemblance to Bismarck, although the author does not appear to have had him in mind.

The typical phallic-narcissistic character is self-confident, often arrogant, elastic, vigorous and often impressive. . . . The facial expression usually shows hard, sharp masculine features. . . . Such individuals usually anticipate an expected attack with an attack on their part. Their aggression is very often expressed not so much in what they say or do as in the manner in which they say or do things. . . . The outspoken types tend to achieve leading positions in life and resent subordination unless they can—as in the army or other hierarchic organizations—compensate for the necessity of subordination by exerting domination over others who find themselves on lower rungs of the ladder. If their vanity is hurt, they react either with cold reserve, deep depression or lively aggression. . . . In spite of their narcissistic preoccupation with their selves they often show strong attachments to people and things outside. . . . In relatively unneurotic representatives of this type, social achievement, thanks to the free aggression, is strong, impulsive, energetic and usually productive.

Analysis of such types usually shows an “identification of the total ego with the phallus,” “serious disappointments” at the genital stage in the relationship to the mother, and a home in which the mother was the stronger parent. “Whether such a type turns into a creative genius or a large-scale criminal depends largely on the social atmosphere and the possibilities it provides for an outlet of the energy in a sublimated form.”²⁰

Reich was of the opinion that character formation begins with the attempt of the child to solve the Oedipus complex. He took for granted the findings of Freud and his school concerning the different character types that emerge during the oral, anal, and genital stages of infantile development, but stressed that character is formed less by the libidinal drives themselves than by the defensive “armoring” that occurs when the child develops anxiety concerning those drives owing to the repressive intervention by a parental figure. Unlike other types, the response of the phallic-narcissistic character to such an experience is not regression. “He remains at the phallic stage; more than that, he exaggerates its manifestations *in order to protect himself against a regression to passivity and anality*.”²¹ Undoubtedly Bismarck did display a rather exaggerated masculinity. Consider, for example, his dueling record as a university student (twenty-five bouts in three semesters); his frequent boasts about his drinking capacity (six bottles of wine without getting drunk or sick) and his occasional exhibitionism

²⁰ Wilhelm Reich, *Character Analysis* (3d ed.; New York, 1949), 200–06.

²¹ *Ibid.*, 203–06. The italics are in the original.

(quaffing an entire bottle of wine without pause before the officers of the Magdeburg cuirassiers); his youthful recklessness as a horseman (he claimed to have fallen fifty times);²² his frequent depictions of himself as a "soldier" in God's and the king's service; the general's uniform he wore after 1866 as his normal dress; and the huge and vicious dogs that were his constant companions.

It may be, furthermore, that there was more to the famous dream he recounted in a letter to the Kaiser in 1881 and repeated later in his memoirs than Freud and Hanns Sachs, who analyzed it in 1913, realized:

Your Majesty's communication encourages me to relate a dream which I had in the Spring of 1863, in the hardest days of the Conflict, from which no human eye could see any possible way out. I dreamt (as I related the first thing next morning to my wife and other witnesses) that I was riding on a narrow Alpine path, precipice on the right, rocks on the left. The path grew narrower, so that the horse refused to proceed, and it was impossible to turn round or dismount, owing to lack of space. Then, with my whip in my left hand, I struck the smooth rock and called on God. The whip grew to an endless length, the rocky wall dropped like a piece of stage scenery and opened out a broad path, with a view over hills and forests, like a landscape in Bohemia; there were Prussian troops with banners, and even in my dream the thought came to me at once that I must report it to your Majesty. This dream was fulfilled, and I woke up rejoiced and strengthened.

In his interpretation Sachs pointed out that the dream contains two layers of wish-fulfillment: a fantasy of childhood masturbation executed "in the face of prohibition" and a desire to escape from the dilemma of the Prussian constitutional conflict through war against Austria.²³ In the context of a phallic-narcissistic character the dream might also be interpreted as revealing an "identification of the total ego with the phallus." It is with the phallus, "an instrument of aggression and vengeance" in Reich's model, that Bismarck overcomes all obstacles and opens the way to military conquest.

Narcissism was a central feature of Bismarck's personality. Nevertheless he was a more complicated person than Reich's model, and he did not possess all of its attributes. One attribute is orgasmic impotence, which was no more true of Bismarck than of Napoleon and Mussolini, whom Reich identifies as phallic-narcissistic characters. We shall see, furthermore, that Bismarck did exhibit regressive behavior during his greatest personal crisis as an adult; yet this too can only be understood in terms of narcissism, as is shown by more recent contributions to the study of character formation. Writers on psychoanalytic theory are now less inclined than Reich to assume

²² See Moritz Busch, *Tagebuchblätter* (Leipzig, 1902), 1: 373-74, 381, 580; Bismarck, *Werke*, 8: 498, 516; Heinrich von Poschinger, ed., *Bismarck-Portefeuille* (Stuttgart, 1899), 4: 95.

²³ Sigmund Freud, *The Standard Edition of the Complete Psychological Works of Sigmund Freud*, tr. and ed. James Strachey et al. (London, 1953-), 5: 378-81. The translation of the dream used in this edition is, with minor improvements, that of A. J. Butler in *Bismarck: The Man and the Statesman* (New York, 1899), 2: 212-13. For the original letter in German, see Bismarck to William I, Dec. 18, 1881, in Bismarck, *Werke*, 15: 377.

that character is formed so exclusively through the handling of the oedipal problem or through the interaction between instinctual demands and ego defenses. They prefer, moreover, to work with a much wider variety and combination of character traits in order better to explain the uniqueness of individual development.²⁴ Narcissism, furthermore, is generally treated as a common and natural phenomenon of all three stages of ego development. In describing the "mechanism of depression," Edward Bibring, for example, writes:

The narcissistic aspirations developed on the oral level, or subsequently built on it, may be generally defined as the need to get affection, to be loved, to be taken care of, to get the "supplies," or by the opposite defensive need: to be independent, self-supporting. Depression follows the painful discovery of not being loved or not being independent, whenever this discovery regressively evokes the primary feelings of helplessness with regard to the gratification of narcissistic needs. . . .

The phallic phase shows again a different type of ego involvement. Competitive strivings within the oedipal situation are intimately linked up with exhibitionistic and sadistic needs to defeat the rival and to be admired by maternal images or substitutes. In the phallic stage, therefore, the narcissistic aspirations stem mainly from the competitive situation, the wish to be admired, to be the center of attention, to be strong and victorious, not to be defeated, and so forth. Depression may result, e.g., when the fear of being defeated and ridiculed for one's shortcomings and defects, or the fear of retaliation, etc., seem to come true and the ego proves too weak to prevent the inevitable.²⁵

We shall see that what Bibring observed at both the oral and phallic stages has some bearing on Bismarck's experience. But first a word of caution. One of the dangers in using a psychoanalytic model is the temptation to extrapolate from the known the unknown and unknowable. This a psychoanalyst may feel free to do, but not the historian. What Reich is willing to assume on the basis of external characteristics about the total life experience of Napoleon and Mussolini is beyond our reach. The value of the model is not that it permits us to peer into the depths of a personality beyond the limits of the evidence available, but that it enables us to make more sense out of the evidence we have by establishing interrelationships, both actual and possible, that might otherwise go unobserved. We cannot reconstruct the exact nature of the interaction between the infant Bismarck and his parents, but we can discern how it affected him.

BISMARCK WAS BORN ON April 1, 1815, the fourth of six children, only three of whom survived childhood (the others being Bernhard, born in 1810, and

²⁴ See Peter Blos, "Character Formation in Adolescence," *Psychoanalytic Study of the Child*, 23 (1968): 245-63; and Ernst Prelinger and Carl N. Zimet, *An Ego-Psychological Approach to Character Assessment* (New York, 1964).

²⁵ Edward Bibring, "The Mechanism of Depression," in Phyllis Greenacre, ed., *Affective Disorders, Psychoanalytic Contribution to Their Study* (New York, 1953), 37-39. The anal phase has been left out as inapplicable. The anal character is pedantic, ruminative, orderly to excess, circumstantial—hardly Bismarck's attributes.

Malwine, born in 1827). Otto's first five years were spent on the family estates at Schönhausen and Kniephof. In 1822 the family moved to Berlin, and the boys were boarded at the Plamann Anstalt, a private school, where Otto spent his sixth to his twelfth years; thereafter, until age seventeen, he attended in succession the Friedrich Wilhelm Gymnasium and the Graue Kloster Gymnasium in Berlin where he boarded with teachers. In later years Bismarck remembered both his mother and his schooling, except for the two years at the Graue Kloster, with distaste.

My mother was a beautiful woman, who loved external elegance, who possessed a bright, lively intelligence, but little of what the Berliner calls *Gemüth*. She wished that I should learn much and become much, and it often appeared to me that she was hard, cold toward me. As a small child I hated her, later I successfully deceived her with falsehoods. One only learns the value of the mother for the child when it is too late, when she is dead. The most modest maternal love, even when mixed with much selfishness, is still enormous compared with the love of the child. Nowhere perhaps have I sinned more grievously than against my parents, above all against my mother. I really loved my father. When not with him I felt remorse concerning my conduct toward him and made resolutions that I was unable to keep for the most part. How often did I repay his truly boundless, unselfish, good-natured tenderness for me with coldness and bad grace. Even more frequently I made a pretence of loving him, not wanting to violate my own code of propriety, when innerly I felt hard and unloving because of his apparent weaknesses. I was not in a position to pass judgment on these weaknesses, which actually only annoyed me when coupled with *gaucherie*. And yet I cannot deny that I really loved him in my heart. . . . I wanted to show you . . . how much it oppresses me when I think about it.²⁶

At first sight this revealing letter conforms only where the father, not the mother, is concerned to the Oedipus complex, which is the heart of Freudian psychology—that is, love for the mother as the primary “love object,” the source of warmth and nourishment, and toward the father ambivalence born of respect for his authority mixed with jealousy because of his possession of the mother. At age thirty-two a man famous for decisiveness of judgment on people and events, whose writings are singularly clear of fuzziness and obfuscation (except for calculated effect), could not make up his mind how he really felt about his father. Toward his mother he expressed a sense of guilt, but no ambivalence and no love. Yet appearances deceive, and here too Bismarck conforms to the pattern. It is a common phenomenon in psychoanalysis that strong negative feelings toward the parent of the opposite sex turn out to be a “reaction formation” concealing the oppo-

²⁶ Bismarck to Johanna von Puttkamer, then his fiancée, Feb. 23, 1847, in Bismarck, *Werke*, 14: 67. By comparing this letter with the original, an enterprising American historian discovered that some critical passages had been left out by the editors of Bismarck's collected works. See Charlotte Sempel, “Unbekannte Briefstellen Bismarcks,” *Historische Zeitschrift*, 207 (1968): 609–16. Although Bismarck often talked freely about his early childhood, even to relative strangers, he never again, so far as the record shows, spoke about his mother so generously as in this letter to his future wife. See, for example, Bismarck, *Werke*, 8: 207; and Poschinger, *Bismarck-Portefeuille*, 4: 100.

site of what they purport to be. Interpreted in this way, Bismarck's attitude toward his mother has to be read as love rather than hatred.

Erikson has admonished us not to blame mother for all that goes wrong in the interaction between mother and child. Yet Bismarck's complaints about his mother are not the only testimony we have concerning her. One of his childhood playmates, his cousin Hedwig von Bismarck, remembered an uncle speaking of Wilhelmine's "fishlike nature";

She remained for us children also, including her own, an unapproachable adult [*Grösse*]. In my memory the prince's mother lives on as a cold woman relating little to the people about her. I cannot remember ever hearing from her a hearty utterance toward any of us. It was otherwise with Uncle Ferdinand! He always had a friendly word or an amusing joke for us, especially when Otto and I rode on his knees. . . . She was often sick and then listless. The first time I heard the word nervous, so common today, was in connection with this woman. It was generally said that she made her life difficult and still more that of her husband and children by her nervousness.²⁷

Once Bismarck remarked of her, "She spoilt my character."²⁸ He felt rejected and unloved; the very severity of his adverse judgment measures the degree of his childhood frustration and disappointment.

In the dynamics of the parental household there are other clues to the formation of his character. When she married in 1806, Wilhelmine Menck-en, an orphan since 1801, was sixteen, and her husband Ferdinand von Bismarck was thirty-four. The disparity in age was not remarkable for that time, but, by all accounts, the relationship between them was. Contrary to the norm in the German household, she was the dominant partner in the marriage. Ferdinand was a country squire and former cavalry officer who retired from the Prussian army in 1795 after serving with distinction against France. In 1806, when Prussia again went to war, he remained at home with his bride; during the years 1812–15 he did not participate in the war against France, a fact that was held against his son four decades later by political opponents. During their years at Schönhausen and Kniephof Wilhelmine is said to have taken a hand in the management of her husband's properties, with unfortunate results. The decision to lease the estates and move to Berlin in 1822 is said to have been dictated by her desire to return to Berlin society, to secure a better education for her two sons, and to provide them later with the proper connections for successful careers in the state service.²⁹ To the end of his life Bismarck never ceased to lament the change that occurred in his life when at the age of six he was taken away from the fields and stables of Kniephof and enrolled in the Plamann Anstalt. Here is a report of a typical utterance:

Then he spoke about the harsh training that he experienced in his early youth . . . and that perhaps had developed in him the gruff touchiness with which he

²⁷ Hedwig von Bismarck, *Erinnerungen aus dem Leben einer 95 jährigen* (5th ed.; Halle, 1910), 28–31.

²⁸ Quoted in Richmond, "Bismarck at Home," 5.

²⁹ Gustav Wolf, *Bismarcks Lehrjahre* (Leipzig, 1907), 23–26; Marcks, *Bismarcks Jugend*, 40–53.

defended himself against the wrongs he suffered. "At the age of six I entered a school whose teachers were demagogic *Turner* who hated the nobility and educated with blows and cuffs instead of with words and reproofs. In the morning the children were awakened with rapier blows that left bruises, because it was too burdensome for the teachers to do it any other way. Gymnastics were supposed to be recreation, but during this too the teachers struck with iron rapiers! For my cultivated mother, child rearing was too inconvenient and she freed herself of it very early, at least in her feelings." He spoke very bitterly about this.³⁰

The Plamann Anstalt was probably not the "prison" Bismarck remembered. To be sure, the routine was somewhat spartan—twelve hours of instruction daily including considerable training in gymnastics, fencing, and other sports. Yet the founder was a disciple of Pestalozzi and one of its patrons was Wilhelm von Humboldt, the educational reformer of the Stein era.³¹ Wilhelmine chose a "progressive" school for her children, one that stressed her own humanistic values; "the views that I imbibed with my mother's milk were liberal rather than reactionary," Bismarck wrote in his memoirs. In 1832 he finished school, by his own testimony, a pantheist, with republican and German-national sympathies—"a normal product of our state school system." "These impressions remained in the stage of theoretical reflections, and were not strong enough to extirpate my innate Prussian monarchical sentiments. My historical sympathies remained on the side of authority." At the university he thought of joining the *Burschenschaft*, but was repelled by "their refusal to 'give satisfaction,'" their bad manners, and the "extravagance of their political views." Thereafter he associated Utopian theories with poor breeding.³²

In the successive stages of Bismarck's development can be discerned the emergence of an inner conflict whose origin may lie in the oedipal triangle with his parents. The frustration of his relationship to his mother, who failed to provide the warmth and approval he required, and his natural ambivalence toward his father, heightened by the latter's failure to act out his role as the figure of authority in the household, were further complicated by displacement from a friendly environment (associated with the father, his ancestry and social milieu) to a hostile one (associated with the mother, her ancestry and social milieu). As his horizon enlarged, Bismarck became aware of the developing conflict between Junker and bourgeois, conservative and progressive, authority and freedom, which reached a preliminary climax in the revolutions of 1830, the Hambach Festival of 1832, and the Frankfurt putsch of 1833—events that occurred when Bismarck was fifteen to eighteen years old. The personal problem that began in the nursery became immensely complicated and ramified by his widening social

³⁰ To Lucius von Ballhausen and others at dinner, Mar. 30, 1878, in Robert Freiherr Lucius von Ballhausen, *Bismarck-Erinnerungen des Staatsministers Freiherrn Lucius von Ballhausen* (Stuttgart, 1920), 137–38.

³¹ See Ernst Krigar, *Kleine Mittheilungen aus der Jugendzeit des Fürsten Bismarck in der Plamannsches Pensions-Anstalt* (Berlin, 1873); Wolf, *Bismarcks Lehrjahre*, 26–51; Marcks, *Bismarcks Jugend*, 53–65.

³² Butler, *Bismarck*, 1: 1–2, 16.

consciousness in adolescence. Hence it was more or less a foregone conclusion that in 1832 the *Burschenschaft* at Göttingen would lose him as a prospective recruit. Other attitudes of Bismarck that appeared in later years may perhaps be traced to these early experiences: his contempt for men dominated by wives, his contempt for intellectuals (the word "professor" was a favorite derogatory epithet), his hostility toward bureaucratic government and suspicion of *Geheimräte* (his maternal grandfather's career), his proclivity for rising late (the children at the Plamann Anstalt were awakened at 6:00 A.M.), his constant longing for the country and dislike of Berlin, and his preference in agriculture for forests (his mother had ordered the cutting of an ancient wood at Kniephof).

Psychoanalysts tell us that the trauma we all normally suffer in childhood may be repressed but never extinguished. It may be that Bismarck's resumption in 1847 of the career he had abandoned in 1838 a few months before his mother's death stemmed from an unconscious wish to fulfill her ambition for him, from his desperate need to win the maternal love he felt had been denied him. That the "man of blood and iron" was plagued by enduring internal stresses he himself admitted to an aide and personal friend, Robert von Keudell. "Faust complains about having two souls in his breast, but I harbor a whole crowd of them and they quarrel. It is like being in a republic. . . . I tell most of what they say, but there are also whole provinces into which I will never let another person look."³³ To another close associate, Christoph von Tiedemann, he was a strange "mixture of hard, iron energy and childish softness."³⁴ "There is no good picture of me," Bismarck once complained. All the artists had painted a "forceful expression." "Actually I have a dreamy, sentimental nature."³⁵ This softer, more vulnerable side to Bismarck's personality can also be seen in his health problem of the 1870s and 1880s.

AFTER THE AGE OF FORTY Bismarck complained with increasing frequency of bad health. During the constitutional conflict and the Danish War (1862–64), he fluctuated between periods of intense activity and near incapacitation. After 1865 the intervals of ill health increased in frequency and duration, reaching a climax at the end of the 1870s. He often doubted whether he would be able to continue in office and twice, in 1869 and in 1875, tendered his resignation on grounds of continuing illness. His afflictions compelled him to surrender the office of Prussian minister-president during the years 1872–74. Some of his problems stemmed from physical injuries. Always an indifferent horseman—observers noted that he did not conform to the movements of the horse—he was in his

³³ Bismarck to Robert von Keudell, Aug. 18, 1865, in Bismarck, *Werke*, 7: 101.

³⁴ Christoph von Tiedemann, *Sechs Jahre Chef der Reichskanzlei unter dem Fürsten Bismarck* (2d ed.; Leipzig, 1910), 212.

³⁵ Lucius, *Bismarck-Erinnerungen*, 56.

early years a reckless one. Three of the many falls he experienced resulted in bad concussions, and the last, in 1868, broke three ribs. Only one of these accidents left a lasting injury, which was caused more by medical mistreatment than by the fall itself. The fall, which left him with a painful left leg, occurred while he was hunting in Sweden in 1857. Two years later a plaster applied by a quack doctor in St. Petersburg ate into his flesh and destroyed a vein. The resulting wound nearly cost him a leg, and complications—thrombosis, embolism, and pneumonia—that developed some months later endangered his life. A bullet fired by a would-be assassin in 1874 left his right hand numb and weak. In addition, Bismarck was afflicted increasingly with neuralgia, rheumatism, gout, migraine headaches, gall stones, and varicose veins, as well as with occasional bouts of grippe, jaundice, shingles, hemorrhoids, gastric disturbances, constipation, and stomach aches and cramps that might have indicated ulcers. From 1866 onward he complained of recurrent facial pains—"as though a sword were being shoved through my cheek, now from the right, now from the left."³⁶ There was a suspicion of bad teeth,³⁷ but his physician Ernst Schweningen diagnosed the ailment as trigeminal neuralgia (*tic douloureux*).³⁸

According to Bismarck and his physicians his greatest health problem was bad nerves. Medicine has always had fashionable ailments, and "bad nerves" was in vogue during the last half of the nineteenth century. In May 1872 Bismarck felt that he was on the verge of collapse, complaining of "this pressure on my brain that makes everything that lies behind my eyes often seem like a glutinous mass. I am not able to hold on to my thoughts continuously. In addition, there is this unbearable pressure on my stomach, with unspeakable pains."³⁹ His physician at the time, Heinrich Struck, diagnosed "functional disturbances in almost all bodily organs," resulting from "excessive use of nervous strength." The daily withdrawal of strength from the "capital" of his nervous system had been far greater than the deposit. Struck feared a "premature marasmus."⁴⁰

While Bismarck had real physical ailments, it is apparent that his health problem was chiefly psychic. His collected works, particularly the recorded conversations, are so studded with complaints about the state of his health as to suggest hypochondria. People who were often in his company during the 1870s and 1880s confirm it, as the following account shows:

He was very sensitive to bodily pain and discomfort, from which he suffered severely, even to the point of tears. He complained a lot, at times in an exaggerated

³⁶ Tiedemann, *Sechs Jahre*, 484.

³⁷ See Busch, *Tagebuchblätter*, 3: 93-94; and Bismarck, *Werke*, 8: 499.

³⁸ Ernst Schweningen, *Dem Andenken Bismarcks* (Leipzig, 1899), 35; and Schweningen, "Blätter aus meiner Erinnerung," in Arthur von Brauer, Erich Marcks, and Karl Alexander von Müller, eds., *Erinnerungen an Bismarck* (Stuttgart, 1915), 187-88.

³⁹ Bismarck to Gustav von Diest, May 11, 1872, in Bismarck, *Werke*, 8: 37.

⁴⁰ Bismarck to William I, in Bismarck, *Werke*, 6c: 19.

way. At such times he had a need to be sympathized with by those around him and to be treated lovingly, and he was bitter if that did not happen in sufficient measure. Often he let himself be diverted by friendly words, jokes, and expressions of sympathy; then he forgot about his complaints. He had a very peculiar attitude toward medicine and doctors. He was very conscious of everything that affected his body, had himself regularly weighed and measured, always had a house physician who visited him every day and who for a long period actually had to put him to bed. He had a lively interest in medicines and, if anyone had a headache, gladly gave him a powder. His chief ailment was a stubborn sleeplessness that was treated for a time with opium, later with paraldehyde, also with baths and wet cloths [*Wicklungen*].⁴¹

At a parliamentary soiree in May 1872 Bismarck described to his guests what happened on a typical sleepless night.

I can't sleep at all, regardless of what I try. I read then, get out of bed again, walk about in my quarters, smoke—nothing helps. It is often seven o'clock in the morning before I fall fast asleep. Then I often sleep until two o'clock in the afternoon. I know that my nerves are at fault—I left them at Versailles. The shameful part of it is that, when I can't sleep, I recall all the vexations I have had—and in increased measure—never anything pleasant. I find excellent replies to utterances that have annoyed me. This wakes me up all the more, and I don't come to a peaceful sleep.⁴²

Among the vexations that kept him awake, he confessed to Lucius in 1876, was the memory of injustices suffered at the hands of those rapier-wielding teachers at the Plamann Anstalt who "hated the sons of noble-men." "Then I get downright hot about it and dream in my half-sleep how to defend myself."⁴³

Hypochondria and insomnia were accompanied by gluttony. Those who could afford it probably ate more in the nineteenth century than is common today, yet even contemporaries were astounded by what they observed at the Bismarck family table. Tiedemann has left a humorous account of an average meal.

The first time I dined at the Bismarcks [1876] he complained about not having any appetite. Then I watched with growing astonishment as he devoured a three-man serving of every course. He preferred heavy and indigestible foods, and the princess supported this inclination. If he suffered from an upset stomach at Varzin or Friedrichsruh, she had nothing more pressing to do than to telegraph Borchardt [a delicatessen] in Berlin for a shipment of *paté de foie gras* of the larger sort. When this was presented at table on the following day, the prince would open a large breach in it with the first stroke. As it was passed around the table he followed it with jealous glances, the consequence being that everyone

⁴¹ "Bismarcks Persönlichkeit, Ungedruckte persönliche Erinnerungen," *Süddeutsche Monatshefte*, 19 (1921): 114–15. The editor of this journal did not reveal the name of the author of the memoir, and I have not been successful in identifying him. The document has the ring of authenticity. Compare Hugo Graf Lerchenfeld-Koefering, *Erinnerungen und Denkwürdigkeiten, 1843 bis 1925* (Berlin, 1935), 241–42.

⁴² Quoted in Heinrich E. Brockhaus, *Stunden mit Bismarck, 1871–1878* (Leipzig, 1929), 53–54.

⁴³ Lucius, *Bismarck-Erinnerungen*, 85.

present remembered the saying "Modesty is a virtue." . . . When the dish came back to him, its volume only slightly diminished, he devoured all that remained. No wonder that after such culinary excesses gastric disturbances were the order of the day.⁴⁴

In 1880 Tiedemann reported to his wife from Friedrichsruh, "They eat here as always until the walls burst." Lunch consisted of "roast beef or beef steak with potatoes, cold roast venison, fieldfare, fried pudding, etc.," dinner of "six heavy courses plus dessert."⁴⁵ The final meal ("tea") came at midnight just before retiring, and obviously had something to do with the host's insomnia. During the 1850s and 1860s Bismarck was a chain smoker of cigars from morning to night.⁴⁶ Later he switched to a pipe and a more temperate pace. According to one report, he drank wine with every meal (including breakfast—along with milk and lemon water), beer or sparkling wine during his afternoon ride, and beer during the evening. Guests were impressed by the amount of wine and beer their host consumed at parliamentary dinners and soirees.⁴⁷ Increasing corpulence compelled him, so he complained in 1878, to purchase a new wardrobe each year.⁴⁸ At age thirty-two Bismarck, who was tall and broad shouldered, weighed 200 pounds (182 *Pfund*), which was presumably his normal weight. At sixty-four he weighed 272 pounds (247 *Pfund*).⁴⁹

Bismarck's attitude toward medicine and doctors was indeed peculiar. While for many years he had one or two physicians in frequent attendance, he professed to have little faith in them, their diagnoses, and their prescriptions. They were "dolts" who "made elephants out of gnats"; the reverse of what they advised was usually correct. The doctors, on the other hand, found him a poor patient. He demanded that they restore his health without changing his life style. One who treated him in 1856

⁴⁴ Tiedemann, *Sechs Jahre*, 484; see also Lucius, *Bismarck-Erinnerungen*, 53-54; and Moritz von Blanckenburg's comment to Albrecht von Roon, in Heinrich von Poschinger, ed., *Fürst Bismarck und die Parlamentarier* (Breslau, 1894-96), 2: 67-68.

⁴⁵ Tiedemann, *Sechs Jahre*, 417.

⁴⁶ At the age of seventy he estimated his lifetime consumption at "not more than 100,000 cigars and 10,000 bottles of champagne." "During my diplomatic period I not infrequently drank two bottles of champagne at mid-day." Bismarck to Rudolf Ihering, Mar. 28, 1885, in Bismarck, *Werke*, 8: 516.

⁴⁷ See "Bismarcks Persönlichkeit," 121. The Bavarian envoy in Berlin, Count Lerchenfeld-Koefering, recalled in his memoirs an evening when Bismarck, after having consumed "several liters of Bock beer" during a soiree, invited him to remain after the other guests had left to drink alternately Jamaica rum and champagne, which were topped off at 2:00 A.M. with port wine. The host became animated and talkative, but appeared otherwise unaffected. *Erinnerungen und Denkwürdigkeiten*, 242-43.

⁴⁸ Poschinger, *Fürst Bismarck und die Parlamentarier*, 1: 139. While draining glasses of Münchener beer "in the genuine student manner," he asked his guests for advice on how to rid himself of podagra and take off weight.

⁴⁹ Georg Schmidt, *Schönhausen und die Familie von Bismarck* (2d ed.; Berlin, 1898), 5; Schweninger, *Dem Andenken Bismarcks*, 25. According to another source, Bismarck's weight varied as follows: in 1874 227 lb., in 1875 241 lb., in 1876 253 lb., in 1877 253 lb., in 1878 267 lb., in 1879 272 lb., in 1880 261 lb., in 1881 255 lb., in 1883 222 lb., in 1885 225 lb., in 1886 227 lb., in 1887 227 lb., in 1888 227 lb., and in 1890 227 lb. Augustin Cabanès, "La Médecine Anecdotique: Bismarckiana," *La Chronique Médicale*, 5 (1898): 534.

described him as “hysterical as a woman [*Frauenzimmer*].”⁵⁰ An incident that occurred one evening in March 1880 is instructive. After consuming “endless quantities of iced May wine” and six hard-boiled eggs spread with butter, Bismarck vomited repeatedly and suddenly began to stammer, complaining “of paralysis in the cheek muscles and back of the head.” He believed he had suffered a stroke and persisted in this belief even after Struck and a specialist assured him that the source of his difficulty was “stomach influenza.” “More obstinate than ever,” he rebuked Struck severely and, disregarding the doctor’s instructions, proceeded to eat a heavy meal and go for a walk in the rain.⁵¹ Struck never really succeeded in controlling his famous patient. Fear of a stroke led to a more normal routine in the Bismarck household and to spasmodic efforts to follow a more sensible diet. Yet it was not until 1883 that Ernst Schweningen, a thirty-three-year-old Bavarian doctor, succeeded in imposing a healthy regimen. He reduced Bismarck’s consumption of wine and tobacco, insisted on sensible hours and proper exercise, and introduced a low-fat diet chiefly of fish, including herring, of which the patient became particularly fond. For the next fifteen years he attended Bismarck almost daily and undoubtedly prolonged his life. Both Struck and Schweningen profited from their relationship with Bismarck, the former becoming head of the Reichsgesundheitsamt, the latter being appointed professor of medicine at Berlin over the protests of the medical faculty. In addition, Schweningen became a prominent society doctor, although never highly regarded by the medical profession.⁵²

From the psychoanalytic standpoint Bismarck’s relationship to his physicians is highly significant. While unable to do without Struck, the chancellor was unwilling to submit his body to the doctor’s control. Why Bismarck submitted to Schweningen has been the subject of considerable speculation. In a memoir published in 1899 Schweningen attributed his success to Bismarck’s discovery that his health soon improved under the new regimen and that any

⁵⁰ Bismarck, *Werke*, 7: 264–65; 8: 22, 286, 389.

⁵¹ Bismarck, *Werke*, 8: 357–58; Tiedemann, *Sechs Jahre*, 393–94. The specialist was Ernst von Leyden, a prominent internist among whose patients were Tsar Alexander III, the shah of Persia, Gerson Bleichröder, and Alfred Krupp. Years later Leyden recalled the diagnosis of Bismarck’s ailment on this occasion as an “overloaded stomach.” His attempts to treat the chancellor during the following months were as unsuccessful as those of Struck. Wilhelm Treue, *Mit den Augen ihrer Leibärzte* (Düsseldorf, 1955), 297, 393–94.

⁵² Schweningen had been dismissed from the medical faculty at Munich on a morals charge (“stuprum in a churchyard under blooming lilacs”). On Bismarck’s intervention he was “rehabilitated” by the Reich government and appointed to the Berlin medical faculty by the Prussian Kultusminister Gustav von Gossler. His presence in Berlin was regarded as “in the interest of the Reich” because of his success in treating the chancellor. Lucius, *Bismarck-Erinnerungen*, 301. Schweningen regarded medicine as an art rather than a science and was skeptical of the results of medical research, which explains his difficulties with his professional colleagues. See his *Der Arzt*, vol. 7 of *Die Gesellschaft, Sammlung Sozialpsychologischer Monographien*, ed. Martin Buber (Frankfurt a.M., 1906). On his professional reputation see the remarks of Sigmund Freud in *The Origins of Psychoanalysis: Letters to Wilhelm Fliess, Drafts and Notes, 1887–1902*, ed. Marie Bonaparte, Anna Freud, and Ernst Kris; tr. Eric Mosbacher and James Strachey (New York, 1954), 245.

return to his old habits was sure to be punished by a relapse.⁵³ But this begged the question, for Struck too had advised changes in hours and diet to little avail. Years later Schweningen confided to a Bavarian historian what occurred on the crucial evening when Schweningen won Bismarck's confidence.

Bismarck was on the verge of a physical collapse. He believed that he had already had one stroke and suffered from severe headaches and complete sleeplessness. No treatment had done him any good. He mistrusted all doctors. One of his relatives, he told Schweningen, had taken his life because of a similar disorder; "that will also be my fate." "Tonight, Your Highness," said the doctor, "you will sleep." "We shall wait and see," Bismarck replied skeptically. Schweningen wrapped him in a damp body roll [*Leibwickel*] and gave him some drops of Valerian, telling him, however, that it was a sleeping potion. Then the doctor sat in the easy chair next to his bed and took one of Bismarck's hands in his own, "like a mother with a restless child," until the chancellor fell asleep. When he awakened in the morning, the doctor still sat at his side and Bismarck could not believe that it was day and that he had actually slept the entire night. "From that moment he trusted me."⁵⁴

Schweningen, it would seem, became a surrogate parent. Whatever the doctor lacked as a physician he made up as a practical psychiatrist who knew instinctively what the patient required. Schweningen moved into the chancellor's residence in the Wilhelmstrasse and became a familiar figure in the household, carefully monitoring what the prince ate, drank, and smoked, how he exercised, and when he retired at night. Putting the iron chancellor to bed evidently became a nightly routine. Once when the doctor was away, the prince remarked at bedtime to his daughter, Marie, "I long very much for Schweningen." While Bismarck often complained of Schweningen's "tyranny," he submitted to it. "I treated the other doctors," he joked, "but Schweningen treated me."⁵⁵

IT IS A COMMONPLACE of psychoanalysis that there is an intimate association between love and food in the relationship of mother and child and that gluttony in later years can often be traced to disturbances in the maternal relationship. In combination with hypochondria and insomnia it is indicative of a depressed state, which, though triggered by a contemporary event, has its origin in a trauma of early childhood. This phenomenon

⁵³ Schweningen, *Dem Andenken Bismarcks*, 36–39.

⁵⁴ Karl Alexander von Müller, *Mars und Venus, Erinnerungen, 1914–1919* (Stuttgart), 79. The use of *Wickeln* (a layer of damp cloths covered by a dry outer layer) is a form of therapy still practiced by some physicians in Germany. Like acupuncture in China, it is believed capable of curing a variety of ailments. While Schweningen employed it against insomnia, its actual function may have been to suggest the womb. Valerian is derived from an herb and has tranquilizing properties. Müller was a professional historian. According to his son, Müller was in the habit of making notes of his conversations with celebrities and of using the notes in writing his memoirs. Unfortunately these notes are at present inaccessible. Letter from Otto Alexander von Müller to me, Feb. 2, 1971.

⁵⁵ Schweningen, "Blätter aus meiner Erinnerung," 203, 221.

has been extensively studied by Sandor Rado, a psychiatrist, whose views are summarized as follows:

In the motivational organization of the depressive spell, the unconscious fear of starvation holds a central position. Though genetically related to the deprivation in early life, its reappearance in the adult may be precipitated by current emotional conflicts, particularly the loss of a love object, or by some physiologic disturbance, causing metabolic changes. The depressed patient experiences this loss as irreparable, and in his despair is unable to take proper steps to effect restitution and repair of his psychic balance.

The subject may react in one of two ways: he may, though fearing starvation, deny himself food out of self-punishment, or he may eat to excess and become obese.⁵⁶ Self-punishment was not Bismarck's way.

That Bismarck felt he had suffered deprivation in early years both in his association with his mother and in his school experience is clear. There is also some evidence that these experiences were linked in his mind to the subject of food. One of his frequent visitors recorded, "The prince himself complained about his appetite, which he, as he once said to me, had inherited from his mother, who was a heavy eater."⁵⁷ Among his many complaints about life at the Plamann Anstalt was that the pupils "ate little and poorly."⁵⁸ If Bismarck conformed to the common pattern, the question is what emotional experience in the 1870s could have reactivated the infantile sense of loss and deprivation with its adult symptoms of depression—hypochondria, insomnia, and gluttony.

While his physicians attributed the "bankruptcy" of his nervous system to overwork, irregular hours, and improper diet, Bismarck himself attributed it to political opposition—not that of foreign but of domestic foes. In the scramble of European politics he expected the great powers to further their own interests; this was the natural law of the European political system. Hence in 1866 and 1870 he was able to endure the hardships and fatigue of military campaigns. His assistants were astonished at the "well nigh superhuman" working capacity he displayed during the summer and fall of 1870.⁵⁹ "C'est la guerre," he explained, "all at once I have nerves like wire."⁶⁰ The serious relapses suffered after both wars he blamed on conflicts with the king, generals, ministers, and political parties. "My oil is used up; I can't anymore," he told Lucius in 1872. "It is too much to struggle alone with such colleagues and against the influence of

⁵⁶ Hilde Bruch, "Psychopathology of Hunger and Appetite," in Sandor Rado and George E. Daniels, eds., *Changing Concepts of Psychoanalytic Medicine* (New York, 1956), 186. For a summary of Rado's findings see his "Psychodynamics of Depression from the Etiologic Point of View," *Psychosomatic Medicine*, 13 (1951): 51–55. See also Gregory Rochlin, "The Dread of Abandonment," *Psychoanalytic Study of the Child*, 15 (1960): 451–70.

⁵⁷ Lerchenfeld-Koefering, *Erinnerungen und Denkwürdigkeiten*, 242.

⁵⁸ Bismarck, *Werke*, 7: 465; 8: 207. Another pupil remembered the fare as plain but adequate. Krigar, *Kleine Mittheilungen*, 20–21.

⁵⁹ See Busch, *Tagebuchblätter*, 1: 63.

⁶⁰ Bismarck to Ludwig Bamberger, Aug. 3, 1870, in Poschinger, *Bismarck-Portefeuille*, 4: 50; see also his letter to his brother Bernhard, July 23, 1870, in Bismarck, *Werke*, 14: 780.

the queen." Parliament too made his life unbearable by opposing him on important issues—"such misunderstanding of the circumstances!"⁶¹

What injured Bismarck most, however, was growing opposition from members of his own social class. Increasingly antagonized by his collaboration with liberals after 1866, many Junker conservatives openly broke with his government on the Kulturkampf and other issues in 1872. This alienation cost Bismarck many personal friends, *Duzbrüder* with whom he had been on intimate terms most of his life. They accused him of betraying his social class, of harming its interests and those of the monarchy, of violating the divine order and falling away from God. To Bismarck it was incomprehensible that conservatives would fail to support the government on any major issue of policy. Their opposition to the Kulturkampf in league with Catholics he regarded as "desertion in the face of the enemy."⁶² Those who ought to support him in their own interest and for the welfare of the country, he complained to Baron von Friesen in 1874, actually "harmed him everywhere and left him entirely in the lurch."⁶³ The most savage attacks made upon Bismarck in the mid-1870s came from conservatives rather than democrats, socialists, or Catholics. Led by the *Kreuzzeitung* itself, the conservatives questioned not only Bismarck's policies, but his ethics and his honesty.

"It is in the nature of my profession," Bismarck lamented to Albrecht von Roon in 1872, "that one makes many enemies, but no new friends; one merely loses the old ones."⁶⁴ "He often thinks he is not appreciated enough and is not loved by his own people," Lucius wrote in his diary.⁶⁵ In March 1873 with Gustav von Diest Bismarck discussed at length his deteriorating relationship with conservatives in the Prussian house of lords and spoke morosely of resigning. Diest tried to divert the chancellor by describing a brilliant performance of *Julius Caesar* that Diest had seen on the previous night. Bismarck interrupted him: "But they stabbed Caesar to death." "Yes," said Diest, "but consider also how Caesar mistreated his best friends. . . ." "But still, they did stab him to death," Bismarck replied.⁶⁶ In July 1874 a Catholic, Eduard Kullmann, shot at the chancellor in Bad Kissingen and wounded him. Apparently Bismarck no longer knew what to expect of visitors who called at his office in the Wilhelmstrasse. He loaded the pistol that had lain on his desk for several years, a memento of Karl Blind's attempt to assassinate him in 1866, and, when he sallied forth to face his foes in parliament, he carried the weapon in his pocket.⁶⁷

⁶¹ Bismarck to Lucius, May 5, 1872, in Lucius, *Bismarck-Erinnerungen*, 21.

⁶² Bismarck to Albrecht von Roon, Dec. 13, 1872, in Bismarck, *Werke*, 14: 845. See also *ibid.*, 14: 802, 856-57; 8: 65-71; and Tiedemann, *Sechs Jahre*, 33.

⁶³ Bismarck to Baron von Friesen, Apr. 19, 1874, in Bismarck, *Werke*, 8: 114. This was also his explanation in his memoirs. *Ibid.*, 15: 350-55.

⁶⁴ Bismarck to Albrecht von Roon, Dec. 13, 1872, in Bismarck, *Werke*, 14: 844-45.

⁶⁵ Apr. 18, 1874, in Lucius, *Bismarck-Erinnerungen*, 53.

⁶⁶ Bismarck to Gustav von Diest, Mar. 19, 1873, in Bismarck, *Werke*, 8: 70.

⁶⁷ Lucius, *Bismarck-Erinnerungen*, 61-62; Poschinger, *Fürst Bismarck und die Parlamentarier*, 1: 67.

Once before Bismarck had felt deserted and betrayed by conservatives with whom he had so often "eaten from the same bowl." While Prussian ambassador at St. Petersburg in 1860, he had come under heavy attack from the liberal press and noted that the conservative newspapers made no attempt to defend him. "One ought not to rely on men," he remarked bitterly. "I am thankful for every impulse that draws me within myself."⁶⁸ During the 1870s he withdrew into the circle of his family, spending many months of the year on his distant estates at Varzin and Friedrichsruh. When in Berlin he seldom appeared at court, which he believed was dominated by enemies, or in public, except for parliament and for the dinners and soirees at which he entertained members of parliament. At his own hearth this beleaguered man found the sense of security and dominance that he no longer felt while among his fellow Junkers and his countrymen.

Johanna von Puttkamer was a plain, pious, somewhat retiring person who contrasted sharply with the other women whom Bismarck courted or admired. Laura Russell, Isabella Loraine, Otilie von Puttkamer, Marie von Thadden, and Katharina Orloff are all described, like his mother, as beautiful and spirited. Yet his letters to Johanna before and after marriage in 1847 reveal a genuine emotional attachment; the earliest have the cathartic quality of a sinner confessing to a priest. Three children were born in fast succession, in 1848, 1849, and in 1852. What he required of his wife was what he had missed as a child in his own home—a maternal figure with warmth, simplicity, dependence, and without social aspirations or pretensions. At the beginning of his diplomatic career in 1851 he stated his needs very eloquently in a letter designed to reassure her about the new life ahead.

I did not marry you in order to have a society wife for others, but in order to love you in God and according to the requirements of my own heart, to have a place in this alien world that no barren wind can cool, a place warmed by my own fire-place, to which I can draw near while it storms and freezes outside. And I want to tend my own fire and lay on wood, blow the flames, and protect it and shelter it against all that is evil and foreign.⁶⁹

A search for the kind of nurture provided by a mother is a common psychic phenomenon. Bismarck's problem was complicated by the unconscious need to replace both the mother that was (educated, beautiful, libidinally desirable), whom he sought in his various love affairs, and the mother that was not (warm, loving, and responsive to his physical needs), whom he found in Johanna. By all accounts Johanna had little interest in anything but her husband and children, their health, nourishment, careers, and general welfare. Bismarck's likes and dislikes, his judgments, hatreds, and animosities became hers. Their two sons, Herbert and Wil-

⁶⁸ Bismarck to Alexander von Below-Hohendorf, Aug. 22, 1860, in Bismarck, *Werke*, 14: 561–62.

⁶⁹ Bismarck to Johanna von Bismarck, May 14, 1851, in Bismarck, *Werke*, 14: 211–12.



Bismarck in the family circle at Friedrichsruh about 1893. Standing from left to right: Dr. Chrysander (Schweninger's assistant), grandchildren (sons of the Rantzaus), their tutor, Count Wilhelm (son), Dr. Schweninger, Countess Herbert (daughter-in-law), and Franz Lenbach (portrait painter). Sitting from left to right: Count Rantzau (son-in-law), Count Herbert (son), Lenbach's wife, Countess Rantzau (daughter), Johanna (wife), Bismarck. Illustration courtesy of the Staatsbibliothek-Bildarchiv, Berlin.

helm, and their son-in-law, Count Kuno von Rantzau, became Bismarck's closest assistants, attending to his personal needs as well as to affairs of state. Yet an intimate friend and frequent guest found him, "despite all love," not very interested in the personal lives of his family. "There is in him a peculiar aloofness [*Fürsichsein*] amid wife and children."⁷⁰

While dominant in his own household, Bismarck feared that other men, like his father, did not dominate theirs. His collected writings and conversations display an almost paranoid suspicion of feminine intrigues directed against himself and his policies by the wives of other statesmen, diplomats, ministers, and monarchs.⁷¹ In 1881 he bitterly opposed the marriage of his oldest son, Herbert, then thirty-two, to the Princess Carolath chiefly on the grounds that she was a sister-in-law of Baron Walter von Loë and Count Alexander von Schleinitz, whom the chancellor regarded as enemies in league with the Empress Augusta, with whom he had feuded since 1848. Faced by his father's tears and his threats of disinheritance and even

⁷⁰ Hildegard Baroness von Spitzemberg, *Das Tagebuch der Baronin Spitzemberg*, ed. Rudolf Vierhaus (Göttingen, 1960), 218.

⁷¹ For examples see Bismarck, *Werke*, 6b: 137, 139; 7: 151, 195, 197-98, 214, 692; 9: 126; 15: 135-37; Busch, *Tagebuchblätter*, 1: 439-43; 2: 303-04, 413-22; Tiedemann, *Sechs Jahre*, 214-15, 249-50; Lucius, *Bismarck-Erinnerungen*, 28, 31, 91, 101, 110; Julius Heyderhoff and Paul Wentzcke, *Deutscher Liberalismus im Zeitalter Bismarcks* (Osnabrück, 1967), 2: 38-39.

suicide, Herbert, who had encouraged the princess to divorce her husband, compromised himself by abandoning her. Bismarck preserved the integrity of his family circle so necessary to his own psychic balance, but he sacrificed the happiness of his son, who grew increasingly misanthropic and alcoholic. Herbert was a talented man who had begun a promising career as a diplomat and served after 1886 as foreign secretary under his father. Through him Bismarck evidently hoped to influence the course of German foreign policy after infirmity forced his retirement and perhaps from the grave itself. But the circumstances of the chancellor's break with William II made that impossible, and Herbert, loyal to the end, resigned with his father in 1890.⁷²

Seen from the psychoanalytic standpoint, Bismarck's behavior in the 1870s shows that the emotional cost of his break with old friends and fellow Junkers was considerable. Their rejection of him reactivated trauma suffered long ago, exacerbated his health problem, and led him to seek at hearth and dining table the narcissistic supplies that his injured ego required. Yet he did not abandon the political arena to his foes. His response combined regression with heightened narcissism. Within the refuge of the family, where his omnipotence was unquestioned, he forged the thunderbolts with which to strike down obstructions to his power and policies in the political world. But even here, surrounded by adoring people and constantly accompanied by a faithful and protective dog, his need for love and acceptance remained unsatiated. Hence he ate and drank and demanded sympathy by complaining about his health. Bismarck was composed of more blood than iron.

ONE OF BISMARCK'S remarkable talents was the capacity to convert handicaps into assets. He repeatedly exploited his bad health and nerves in order to gain his will over the king, fellow ministers, and even the Reichstag. At moments of crisis Bismarck often asserted that the frictions of political life were no longer endurable and that he, if opposed, would be compelled to resign, depart for Varzin or Friedrichsruh, and leave the country to its fate. So great was his ascendancy in German government and political life after 1870 that this tactic often worked. Even severe critics of his domestic policy were convinced of his indispensability in foreign affairs. Objections were overcome, policies adopted, ministers dismissed, councilors transferred, even parliamentary decisions reversed because the chancellor said his health demanded it.

On the other hand, Bismarck's frequent irritability exacerbated his relationships with colleagues, subordinates, and parliamentary deputies. Those hour-long nocturnal debates in which the pacing insomniac con-

⁷² See Louis L. Snyder, "Political Implications of Herbert von Bismarck's Marital Affairs, 1881, 1892," *Journal of Modern History*, 36 (1964): 155-69.

jured up his opponents, rebutted their opinions, and supplied them with malignant motives were hardly erased from his consciousness by the rising sun and slumber of exhaustion. They left him bitter and sharp-tongued, cost him the services of able subordinates and colleagues, and imparted a harsh tone to German parliamentary life that was one of his more unfortunate legacies.

Bismarck's physical and mental state compelled him to spend long periods each year on his estates. Varzin was twelve hours—nine by train and then three by carriage—and Friedrichsruh four hours by rail from Berlin. While he was away the telegraph key chattered steadily and couriers came and went. The chancellor liked to believe that he could govern as easily from Friedrichsruh as from Berlin. Yet his lengthy absences from the nerve center of the German government complicated further the very involved bureaucratic relationships, deprived ministers and councilors of the opportunity for quick communication and exchange of information, and often produced misunderstandings as to his policies and intentions. Even while in Berlin Bismarck's ailments compelled him at times to curtail radically his contact with other high officials; for long periods he communicated with them only through adjutants. Jangled nerves and physical weakness often forced him to suspend work or to limit his working day to two hours. During some years he seldom appeared in parliament to defend his policies, giving rise to ironical remarks about the "mythical chancellor." His condition, furthermore, was the source of frequent, unsettling rumors, such as the story that the morphine prescribed by Struck and Schweningen against pain and sleeplessness had made Bismarck a drug addict.⁷³ For nearly twenty years his political allies feared and his foes hoped for his retirement. Yet year after year, through crisis after crisis, he clung tenaciously to power because he could not do otherwise.

Bismarck's character and his personal problems were matters of considerable historical significance. His quest for power became, through the mechanism of projection, that of the Prussian state and German nation. The end result was Königgrätz and Sedan, the reorganization of Germany, and the reconstruction of the European balance of power. To say this is neither to revive the "great man" theory of history nor to disregard the many economic, social, and ideological forces that produced the environment within which he acted, but merely to assert that Bismarck, too, was a historical force of significance and that through him the environment was altered. After 1871 his drive for power continued to transform the internal political life of Germany through the increase of his personal authority within the imperial and Prussian governments, through the manipulation of parliament and its political parties, and through the re-

⁷³ Bismarck, *Werke*, 8: 709. The rumor reached the ears of William II, who in January 1890 asked Schweningen whether it was true. The doctor denied the charge with indignation. Müller, *Mars und Venus*, 80–81.

shaping of the Prussian-German establishment at the end of the 1870s. Bismarck's personality and policies stunted the growth of parliamentary life in Germany and greatly increased the unitary at the cost of the federal principle in its constitutional order—developments that he himself came to deplore. "Nowhere else in the world," wrote Max Weber in May 1918, "has admiration—not even where it has been most unbounded—for the personality of a political figure led a proud nation to sacrifice its own essential convictions to him so completely."⁷⁴

Probably the greatest cost to Germany of nearly three decades of Bismarck's rule was in the caliber of the men who governed Germany once he was gone. The environment he created did not encourage men of independent spirit and judgment to undertake public careers. Later the mediocrity of those who succeeded him and the wreckage their leadership left behind produced a vague hope that history would repeat itself. "Our consolation should be: *Bismarck was a German!*" wrote an editor of the *Süddeutsche Monatshefte* in 1921. "In his person we meet a series of those characteristics that we rightly identify as the roots of German greatness. The people still live from whom he came. Why should it not be able to produce again in our time of need a man who will fulfill our yearning?"⁷⁵

⁷⁴ Max Weber, "Parlament und Regierung im neugeordneten Deutschland," in *Gesammelte Politische Schriften*, ed. Johannes Winkelmann (2d ed.; Tübingen, 1958), 299.

⁷⁵ "Bismarcks Persönlichkeit," 122.

Between Science and Art

A Review Article by DAVID HERBERT DONALD

RICHARD FREEMAN. *Repentance and Revolt: A Psychological Approach to History*. Rutherford, N. J.: Fairleigh Dickinson University Press. 1970. Pp. 247. \$7.50.

G. R. ELTON. *Political History: Principles and Practice*. New York: Basic Books. 1970. Pp. 184. \$5.95.

ROBERT F. BERKHOFER, JR. *A Behavioral Approach to Historical Analysis*. New York: Free Press. 1969. Pp. viii, 339. \$7.95.

HUGH STRETTON. *The Political Sciences: General Principles of Selection in Social Science and History*. New York: Basic Books. 1969. Pp. xii, 453. \$10.00.

THE PHILOSOPHY OF HISTORY is not a subject that has ever attracted the interest of many American historians, and today there is less work being done in this area by professional historians than ever before. Those responsible for training the next generation of historians virtually ignore it. "Philosophy of history" does not even find a place in the index of *In Pursuit of American History* (1970), Walter Rundell's invaluable survey of the training of American historians, and only two or three of the numerous methods courses Rundell describes make even a gesture toward covering the philosophical aspects of history.

Though there are doubtless deep-rooted cultural reasons for this neglect, it requires no profound study to recognize that American historians avoid the philosophy of history mostly because it is of so little assistance to them in writing or teaching. Four recently published books, each dealing in its separate way with the philosophy of history, suggest the difficulties the practicing historian has in deriving sustenance, much less stimulation, from this dry fare.

The least pretentious of these works, but in some ways the most ambitious, is Richard Freeman's *Repentance and Revolt*, an attempt to formulate a psychological interpretation of world history. It would be easy to poke fun at Freeman's book, which is full of factual errors (e.g.,

I am grateful to the members of the History Seminar at Johns Hopkins for constructive criticism of this essay.

Andrew Jackson "introduced the spoils system"), of doubtful generalizations ("In contrast to the Austrian and German chancellors, the British prime ministers were from all classes"), and of truisms ("The modern world has been the scene of war and conquest, and . . . the structure and way of life of certain societies contributed much to this spectacle"). Yet criticism of this sort is essentially beside the point, for Freeman is less interested in detail than in sweeping generalization; he has made a serious attempt to find common patterns in the histories of civilizations, whether ancient or modern, oriental or western. In other words, he has tried to do what Arnold Toynbee did—but in 228 pages.

Freeman classifies civilized societies into three categories: "those which were continually fearful of the surrounding peoples," which he calls migratory; "those which traded with them as well as fought with them occasionally," which he terms interactive; and "those which conquered and ruled over them," which he labels expansive. It is not, however, this typology of societies that interests him so much as the psychological consequences of belonging to each type. Freeman's sympathies are clearly with the interactive societies, like those of the Hellenistic world, which, "being more lenient toward self-gratification," were open to new ideas and broadly tolerant of other peoples. Expansionist societies, on the other hand, are not merely warlike but sadistically cruel, because the "restriction and regimentation" imposed upon their people, "contrary to normal human inclination," produce aggression. But each act of aggression creates a psychological need for atonement, and when societal regimentation frustrates this urge a further impulse toward more aggressive expansion appears. The only escape from this cycle, Freeman suggests, lies in the recent emergence of "neo-migratory societies." With more "education, travel and communication," he hopes, "we can afford to begin living in a more natural and relaxed manner."

There is no need here to attack Freeman's schema of world history. The difficulties of classifying civilizations and of tracing cyclical patterns in their development are familiar to anyone who has studied the criticisms of Toynbee. Moreover, psychology is not such a precise science as to enable Freeman to establish a one-to-one ratio between social organization and personality structure. But for the working historian the main deficiency of Freeman's book is that it has no relevance to any particular historical investigation. If all Freeman's generalizations were true, it is hard to see that they would help a scholar write a better biography of, say, Philip the Fair or a more judicious study of the Agricultural Adjustment Administration. Taxonomy is doubtless a useful, if rudimentary, scientific tool, but it is rarely of much use in the process of explanation, which is the historian's central business.

"Law-making and the discovery of laws are none of the historian's tasks," writes G. R. Elton, as if in reply to books like Freeman's. "It can, in fact, be shown that there can be no laws in history." Elton's essays on *Political*

History, which are an elaboration and reformulation of ideas sketched in his *Practice of History* (1968), reveal a writer wholly averse to schematization, even in his own writing. He is proud of the "relative lack of order" in this little book, because the absence of system "itself reflects realities about historical writing which a more precise and philosophical approach obscures."

Witty and deliberately provocative, Elton's book is an unabashed plea for a return to old-fashioned political history, which, he argues, has primacy over all other branches of the discipline. "Not to be interested in power and politics . . .," he announces, "is not a mature state of mind." Those "historians who can muster no interest for the active political lives of past societies have no sense of history at all." But political history is not for everybody. The "swinging historian" who wants to listen to the voices of the inarticulate had better stay away from it; it must begin with the study of "the affairs of the great." The quantifiers would also do well to turn elsewhere. "Unquantifiable history is more subtle than quantifiable, more variable and more contingent," writes Elton. Anyway, "it has to be said that analyses of voting behavior and such like are not being made to yield better or clearer answers so far by being written in figures and jargon rather than in English." Nor is this the field for philosophers of history, whose work reveals striking "ignorance of what actually happens when a historian investigates the past and attempts to explain it." In short, political history "repels the generalizers, the radicals, and the believers, the men (of a religious temperament) who need proof that history is pointing somewhere, preferably in the direction they want to go." Instead, "political history, more than any other form of historical study, humbles intellectual pride and teaches caution." Those unwilling to be cautiously humble, Elton suggests, might go and "immerse themselves in the thick molasses of social history."

Thoroughly delightful, Elton's *Political History* is also curiously disappointing. It is full of grand generalizations about how political history ought not to be written but offers little useful advice for those who are writing it. Too often Elton's glittering wit conceals commonplace thinking. So dazzling is his demolition of the philosophers who have dared write about historical causation that a reader might almost be blinded to the fact that Elton himself has nothing significant to say on this important subject. "No historian . . . can manage without establishing frequent causal relationships," he offers. One must choose "between knowing the cause beforehand"—as he says Toynbee did, and as he doubtless would say Richard Freeman does—and "empirically discovering it as best he may." But when it comes to explaining this latter method, Elton grows increasingly vague. "A cause is no less a cause for being big and woolly," he asserts. Anyway, "in history, things cannot always be explained." If this is the best advice that one of the major British historians can offer, it is hardly surprising that most American historians have

little interest in the systematic study of the philosophy or methodology of their discipline.

A major exception is Robert F. Berkhofer, Jr., whose *Behavioral Approach to Historical Analysis* attempts to construct a tightly systematic historical methodology.¹ This is a thoughtful and important book. Berkhofer has read extensively in anthropological, sociological, and philosophical literature, and if his book had no other merits, it would serve to bring to the historian's attention the large and often technical literature in these cognate fields. For example, Berkhofer offers a straightforward summary of Clyde Kluckhohn's accounts of how an anthropologist analyzes a culture; he gives a fair and accurate account of the major schools of American sociologists; and he reviews the complex philosophical studies on historical causation, even though he has as little use for these theories as does Elton. Berkhofer is less adequate in presenting the relevant psychological literature because he believes "historians should direct their efforts far more than they do to seeking social and cultural interpretations of their subjects rather than the oversimplified psychology so often urged upon them." Particularly fresh and rewarding are his two chapters on "Conceptions of Time" and "Time and the Discipline of History."

But Berkhofer's book is intended to be far more than a paraphrase of his reading; it is an attempt to formulate "a 'paradigm' of historical analysis for our time." Since historians necessarily, if often unconsciously, generalize even when writing about the unique, Berkhofer wants them to become self-conscious about these generalizations, which too often derive from outmoded theories of human nature and society "denied by years of research and thinking in other disciplines." Instead, they need to incorporate "sophisticated social theory and precise explanation into the writing of history in order to achieve a more complex representation of past reality."

Berkhofer believes that "the approach denominated behavioralism" is the most useful theoretical orientation for the historian. More specifically he favors "situational analysis," which "combines all the factors that enter into human behavioral activity into one order of analysis, whether these be idiosyncratic, rational, cultural, social, psychological, or biological or physical environmental, and places them in relation to the concrete actions of individuals as perceiving, thinking, feeling, acting, and reacting organisms." Though Berkhofer most fully develops this ambitious approach in his discussions of group behavior, it is easier to illustrate the concept here in terms of an individual's actions. Instead of accepting a simple stimulus-response pattern, Berkhofer prefers to view an individ-

¹I initially hesitated to review this book because Berkhofer has singled out some of my writings for sustained and factually inaccurate attack. My scruples vanished, however, when I discovered that Berkhofer lists me along with Frederick Jackson Turner, Richard Hofstadter, and Henry Nash Smith as models that the methodologically sophisticated historian should shun. By placing me in such company he has erased any prejudice I might have had against his book—but has reinforced my doubts about his judgment.

ual's behavior as the result of his situational interpretation of converging social, cultural, and physiological forces, of his perceptions of his physical environment, and of his conscious intentions and unconscious motivations. In discussing how all these very different forces interact, historians should not follow hunches or intuitions; they "should bring to the task explicit theory about the nature of the entities assumed in the actor's internal state and external action, so they can analyze their evidence with greater rigor and frame their questions with more complexity." Presumably psychological theories would be most helpful in thus analyzing an individual's actions; sociological and anthropological theories would be more valuable in the study of group behavior.

In making a situational analysis Berkhofer urges the historian to distinguish between the situation as it was perceived by the participant at the time he acted and as it has been subsequently perceived by posterity. He also warns against permitting situational analysis to drift into "what historians frequently call a multicausal analysis"; the historian's eclecticism "should add up to a dynamic interrelationship of factors, not a mere sequential conjunction of events."

More learned, more complex, and more subtle than a brief summary can suggest, Berkhofer's *Behavioral Approach to Historical Analysis* is nevertheless marred by major flaws. First, it is written in singularly impenetrable jargon. A fairly typical sentence reads: "Although an analyst cannot presume identical total cultural ideation for all members of a group in behavioral interaction, neither should he leap to the conclusion that nothing is shared beyond their behavioral interdependence and the idea that they are a society." In some such words no doubt computers croon love songs to each other beneath the darkling moon.

Berkhofer's argument is further undermined by the awkward fact, which he recognizes, that "there is no *one* behavioral approach, only approaches, because of the many arguments over the fundamental interpretations of human behavior." Thus he is in the embarrassing position of simultaneously urging historians to look to the sociologists for light and of warning that sociology is a battleground between the structural functionalists and the conflict theorists. Lamely he suggests that if historians would explore both sets of theories they would doubtless "profit from their newly gained knowledge."

Another weakness of Berkhofer's case is his inability to point to successful applications of situational analysis to important historical problems. Most of his book is highly abstract, but when he does descend to specifics, the results of his method are unimpressive. When, for example, he writes that among students of mercantilism "the real question that must be investigated and debated is whether the observer's self-conscious system accurately describes a conscious . . . orientation in the actors of the period, and if such an orientation existed, did it produce the scattered laws and policies [called mercantilistic]," he is simply using highfalutin language

to describe what every historian of the subject has been trying to do. Even less impressive is Berkhofer's endorsement of Harry Eckstein's formula "that the problem of causation [of wars] be reduced to an analysis of preconditions and precipitants." It is hard not to distrust the judgment of a historian who bumbles that this "one simple model" does more to clarify the causes of the American Civil War than did Howard K. Beale's meticulous sifting of the vast literature on that topic.

Finally, Berkhofer's argument is clouded by the fact that he is too good a historian to accept all his own conclusions. On the one hand he argues for history as a science, methodologically sophisticated, mathematically precise; on the other, he opposes abandoning "goals traditional to the historical profession for ones espoused by the social scientists or philosophers." He lavishly admires and extensively summarizes Neil J. Smelser's analysis of social change in the Industrial Revolution—only to conclude that it is not a useful model for a working historian, since "the problems posed by the organismic model, difficult enough on a less general level of social organization, become insuperable on the most general level of a total society." Berkhofer is enchanted with the precision and elegance of the new mathematical economic history and he wants historians in other fields similarly to substitute "techniques of quantification . . . whenever possible for impressionistic surveys of materials." At the same time he recognizes that the cliometricians concentrate upon what is measurable, not on what is important, and concludes: "No matter how clever econometric analysis of slavery's profitability may be, historians must still determine what the slaveholders thought on the subject."

Berkhofer's book is worth such extensive evaluation because it is the best case that has recently been made—and perhaps the best case that can now be made—for considering history as one of the behavioral sciences. If the book must be adjudged on the whole a failure, the difficulty is less in the author than in the approach. While some historians can gain insight from the literature that Berkhofer summarizes, none can write good history merely by following his behavioral method.

Hugh Stretton's *The Political Sciences* might almost have been written as a rebuttal to Berkhofer's book, or to the kind of scientific-objective historical writing that Berkhofer advocates. Broadest in scope of all the four books under review here, Stretton's is a vast, rambling, repetitious attack upon behaviorism. Berkhofer's heroes are Stretton's villains, and he charges that Talcott Parsons—one of the sociologists Berkhofer most admires for the strength of his logic and the clarity of his insight—has corrupted "not only . . . students' capacities for clear thought and effective and good-mannered communication, but . . . their ideas of science as well." Stretton fears that the methods that have already done so much damage in sociology, political science, and economics may spread to history.

Historians may be tempted to dismiss *The Political Sciences* without the careful consideration it deserves. For one thing, it deals less extensively with history than with any of the other social sciences. For another, it is badly overwritten, being at least one-third too long. Stretton also plays tricks on his readers. His opening chapter, "Why Men Act," poses as a case study in historical motivation the reasons why Joseph Chamberlain changed his mind in 1899 about old-age pensions, which he had hitherto favored; evaluating the possible explanations for this switch, Stretton sandwiches between quotations from Elie Halévy and R. C. K. Ensor others made up by himself—"composed for the occasion," as he puts it. Again, after elaborately developing a kind of three-dimensional grid to permit the ideological identification of eight types of historians, Stretton lets his readers down by confessing "doubt, that those categories have served any better purpose than to give the contents of this chapter a workable order."

Such capers should not distract readers from the seriousness of Stretton's purpose. By a close, critical examination of the writings of a dozen major social scientists he hopes to demonstrate that the kind of behaviorism that Berkhofer now advocates for history has proved intellectually sterile. The great books in the social sciences have been written, not by behaviorists, but by men like Halévy and Oscar Lewis, whose *Children of Sanchez* (1969) "is as near as social science is likely to come to an objective, value-free report." Such writers owed their success to the fact that, avoiding the false pursuit of "scientific" objectivity, they recognized that their own values led them to select their subjects and to shape their materials. "Values can indeed distort observation and mislead selection," Stretton admits, "but there can be little of either without them. The problem is not how to be rid of values, but is rather how to decide which are best, and how best to apply and employ them."

Here Stretton is back with the old problem of historical relativism. If everyman is his own historian, it is not too hard to argue that every man's history is as good as another's. If a man's personal values lead him to choose his subject, to select his data, and to shape his writing, is not the whole apparatus of historical scholarship—the footnotes, the quotations, the tables and charts, and all the rest—a façade? And is not the process of graduate training in history therefore a charade? These were precisely the questions that Elton, with bluff British common sense, wanted historians to avoid by pretending that they did not exist, that Berkhofer attempted to answer by inventing a new method for truly scientific history. But Stretton remorselessly insists that the problem of objectivity is still there. If his chapters "resume some methodological arguments of the nineteen-fifties," he reminds his readers, "their unoriginality does not make them the less important." The fact that writers on the philosophy of history thus continue endlessly to debate the age-old questions concerning values and

objectivity, concerning causation and explanation, is perhaps reason enough why the historical profession as a whole ignores their books.

Two of the books under consideration hint, however, that historians may finally have had enough of these questions that they have with themselves too much discussed, too much explained, and may be looking toward a new approach to the philosophy of history. After 418 pages of demonstrating why the social sciences are not and cannot be scientific, Hugh Stretton abruptly remarks: "Whatever the value of this present study it needs to be done again with art instead of science as its differentiating model." Social scientists, he thinks, should make serious comparisons of the work of Sinclair Lewis and Robert and Helen Lynd, of "Durkheim's and Faulkner's and Nabokov's understanding of anomie; Donne and Gide and Sorokin upon love; Jane Austen's and David Riesman's judgments of social conformity." A like suggestion, somewhat surprisingly, crops up in Berkhofer's final pages, where he urges historians to "search for new ways of telling their stories"; he hopes for change in the form of historical discourse comparable to the revolution that Joyce and Faulkner wrought in the form of the novel.

Elsewhere in the historical profession, other voices making similar appeals are beginning to be heard. In his bleak survey of the historical profession, presented at the American Historical Association convention in Boston, Oscar Handlin called for experimentation and innovation in the literary form of history. More recently, John Higham's *Writing American History* (1970) has lamented the lack of training given graduate students in history as a literary art. At the very time the Committee on the Beveridge Award of the American Historical Association was taking a backward step by favoring "books that employ new methodological or conceptual tools," Higham was warning that overemphasis upon quantitative and analytical methods has had a serious negative impact upon American historical scholarship, and he urged universities now to concentrate upon educating the "artistic and moral sensibilities" of young historians.

The point, of course, is not to insist that history ought to be well written (though it ought), nor even to argue that history is a branch of literature (though it certainly is). Instead, it is to urge that the time has come for a historians' declaration of independence. No longer should a historian reveal secret shame for his work by attempting to pass his books off as "scientific" or by trying to make them "as interesting as a novel." The true standards for judging the historian's work are not to be derived from natural science, nor philosophy, nor literature. Instead, as in any other art, they are autochthonous. When writers on the philosophy of history and on historical methodology take as their next assignment the formulation of such values, they will, for the first time, find both receptive readers and faithful followers in the historical profession.

A. G. Dickens and the Men of the Sixteenth Century

A Review Article by THOMAS E. MORRISSEY

A. G. DICKENS. *The Counter Reformation*. (History of European Civilization Library.) [New York:] Harcourt, Brace and World. 1969. Pp. 215. \$2.95.

A. G. DICKENS. *The English Reformation*. Reprint; New York: Schocken Books. [1968.] Pp. x, 374. \$2.45.

A. G. DICKENS. *Reformation and Society in Sixteenth-Century Europe*. (History of European Civilization Library.) [New York:] Harcourt, Brace and World. 1966. Pp. 216. Cloth \$5.50, paper \$2.95.

A. G. DICKENS. *Lollards and Protestants in the Diocese of York, 1509-1558*. Reprint; New York: Oxford University Press for the University of Hull. 1966. Pp. 272. \$5.60.

A. G. DICKENS. *Thomas Cromwell and the English Reformation*. (Men and Their Times.) Rev. ed.; London: English Universities Press; distrib. by Lawrence Verry, Mystic, Conn. 1967. Pp. 192. \$3.50.

A. G. DICKENS. *Martin Luther and the Reformation*. (Men and Their Times.) London: English Universities Press; distrib. by Lawrence Verry, Mystic, Conn. 1967. Pp. viii, 184. \$3.50.

A. G. DICKENS and DOROTHY CARR, editors. *The Reformation in England: To the Accession of Elizabeth I*. (Documents of Modern History.) New York: St. Martin's Press. 1968. Pp. vii, 167. \$5.00.

AFTER MANY YEARS of research and writing on the Reformation era and the period surrounding it, it was only natural that Professor A. G. Dickens would finally turn to attempting a comprehensive look at and portrayal of that peculiarly complex phenomenon, the Counter Reformation. Since by some anomaly or mishap his earlier studies had not been given due attention in this journal, it is fitting to use this occasion to review his achievement in portraying the world of the sixteenth century. I shall concentrate on *The Counter Reformation* as well as on two other major works, *The English Reformation* and *Reformation and Society in Six-*

teenth-Century Europe. Since the two earlier works set the stage in one sense for the later one, I shall consider them in chronological order.

The English Reformation has justly been considered a magisterial work. Professor Dickens had earlier completed a number of studies on pre-Reformation and Reformation England; these were for the most part detailed studies covering a restricted area or problem;¹ in addition he had edited a series of texts from this period.² He also published a synthesis of this research in regional history, as opposed to merely local history, in *Lollards and Protestants in the Diocese of York, 1509–1558*, a book that marked the turning point as he shifted his interests onto a broader scale.

Because he studied Yorkshire in detail, Professor Dickens was well aware of and deeply involved in “grass roots history.” He had seen beyond the courts and parliaments, the palaces and capitals, to the towns and villages. He had investigated how laws and decisions affected people in all walks of life who lived far from the centers of power. He had also seen how little or how much the local people enforced the laws, interpreted the decisions, and carried out the changes. Professor Dickens knew the people, the places, and the times of Reformation England, and so it was to be expected that his skill and knowledge would not fail him when he described the event as a whole.

Dickens carefully notes that a ground swell of critical dissent swept away the entire fabric of the medieval ecclesiastical society in England. This dissent came from a variety of sources: the *devotio moderna*, Lollardy, hostility to the Church in London and in other cities, Christian humanism, and the merchant classes and their connection with Lutheranism. He remarks that the clerical society having to face this threat was itself divided by yawning chasms between Wolsey and the bishops, between the upper and the lower clergy, and between the regular and the secular clergy (p. 57). Professor Dickens vividly portrays the fundamental split: Commons saw only the evil aspects and abuses, while churchmen clung to every privilege.

¹ For example, “Aspects of Intellectual Transition among the English Parish Clergy of the Reformation Period: A Regional Example,” *Archiv für Reformationsgeschichte*, 43 (1952): 51–70; “Secular and Religious Motivation in the Pilgrimage of Grace,” in *Studies in Church History IV: The Province of York* (Leiden, 1967): 39–64; “Heresy and the Origins of English Protestantism,” in *Britain and the Netherlands*, vol. 2: *Papers Presented to the Anglo-Dutch Historical Conference 1962* (Groningen, 1964), 47–66; Robert Holgate, *Archbishop of York and President of the King's Council in the North* (London, 1955); “Archbishop Holgate's Apology,” *English Historical Review*, 56 (1941): 450–59; “The Marriage and Character of Archbishop Holgate,” *English Historical Review*, 52 (1937): 428–42; “The Yorkshire Submissions to Henry VIII, 1541,” *English Historical Review*, 53 (1938): 267–75; “Robert Parkyn's Narrative of the Reformation,” *English Historical Review*, 62 (1947): 58–83; “The Edwardian Arrears in Augmentations Payments and the Problem of the Ex-Religious,” *English Historical Review*, 55 (1940): 384–418; “Two Marian Petitions,” *Yorkshire Archaeological Journal*, 37 (1948–51): 376–84; “The Extent and Character of Recusancy in Yorkshire, 1604,” *Yorkshire Archaeological Journal*, 37 (1948–51): 24–48; “The First Stages of Romanist Recusancy in Yorkshire, 1560–1590,” *Yorkshire Archaeological Journal*, 35 (1940–43): 157–82.

² For example, *Three Tudor Treatises: Sir Francis Bigod, Robert Parkyn and Michael Sherbrook* (Leeds, 1959); *The Register or Chronicle of Butley Priory Suffolk* (Winchester, 1951); *The Clifford Letters of the Sixteenth Century* (Durham, 1962); and finally, Dickens and Carr, *The Reformation in England to the Accession of Elizabeth I*.

Certain significant aspects of this crisis were more peculiar to England than to other lands at this time. First of all, England, in contrast to the other lands, did not have a vernacular Bible, and the hostility of the episcopate to allowing the people to read the Bible was constant and enduring (see page 189 for what the bishops considered "concessions" as late as 1543). This hostility is in part explained by the fact that rewards in the Church in England went to clerks who were primarily lawyers rather than pastors. Professor Dickens is able to characterize the episcopate as a body as "the most cautious and unimaginative in Europe." They were civil servants who, to the end, thought of the Church in terms of jurisdiction rather than the needs of religious education. It is no wonder that in one of his studies Dickens is able to refer to the "stodgy episcopate."

With this situation, change was inevitable. But Dickens is very careful to point out that the road from Royal Reformation to the later and quite different Protestant Reformation was not so straight as has often been thought, nor was it equally inevitable. He sees the Royal Reformation as a twofold process: first, the conditioning of society to a rule of law and then, on the religious level, the severance of English society from the papacy and the curtailment of the privileges and wealth of the Church. Only at the end did the door open to the next and different step, the transformation into a Protestant society. As he shows, this final stage at times faced strong opposition from the king in the two decades from the fall of Wolsey to the death of Henry.

Professor Dickens sees Lollardy performing a dual function in that it hardened the episcopate against all change while it served as a spring-board for critical dissent. He stresses the influence of Marsiglio of Padua on royalists as disparate as Gardiner and Cromwell. The latter is given credit for the use and dissemination of the work of this famed Italian critic of the papacy. Dickens is aware of the problems discussed recently concerning the rise of the gentry in the sixteenth century, and he wonders how much the state of the clergy changed during this period, what function the monastic lands served in this, and to what extent they were basis for broadening the group that composed the gentry from whom in large part the clergy would come.

Professor Dickens argues in dispassionate fashion and yet convincingly for the re-examination of some previously accepted views. In earlier studies he had given a new evaluation of the Pilgrimage of Grace. He would also have historians make a fairer assessment of Henry VIII and of the position of the dynasty in the 1520s when it became clear that Henry would have no son from Catherine. He gives a sympathetic portrait of Cranmer as the latter's mind wended its tortuous path in the intricacies of eucharistic theology. He makes it clear that, while from 1532 to 1540 there was a struggle between two groups around the king—the radicals led more or less by Cromwell and Cranmer, and the conservatives represented by Norfolk and Gardiner—both groups were strongly Henrician.

In a figure like Latimer and the "Commonwealth men," the social critics of their time, he sees a linking of the ideas of men such as More, Erasmus, Cromwell, and Starkey, and he notes that these critics were protected by Cromwell and Somerset when each was in power. One query might be made concerning his treatment of the Forty-Two Articles. He seems to accept the view that these articles were intended to serve as a *via media* between the extremist claims of Romanism and Anabaptism. This would seem to give them a very nebulous content and make them latitudinarian far beyond the mind of the sixteenth century.

Professor Dickens is able to give a proper evaluation to events in Marian England. He shows that the accession of Mary was a victory for Henrician principles, for it meant that Tudor England cared more for peace, unity, and lawful succession than for religious causes. He makes it clear that while many of the leaders in Edward's reign had done much harm to the cause of the Protestant Reformation, it was a series of blunders and an inability to understand the mood of the nation on the part of Mary and her chief adviser, Cardinal Pole, that ensured the victory of the Protestant cause. First of all, it was the Spanish marriage, which Pole, incidentally, had strongly opposed, that ensured Elizabeth's succession; the Spanish tie led to the war with France and caused problems in Rome and weakened Pole's position there. More important, Mary relied on Pole who had been away from England for so long. In the next reign the martyrs would mostly be celibate priests educated on the Continent. A circumstance that helped to turn the people against the government's policy under Mary was that the martyrs were more often from the common people and with families. It is with no sense of derogating from their personal commitment that at one point Dickens refers to the Protestant martyrs and the Catholic traitors. This was merely how they were viewed in the eyes of the law and of the people.

A more specialized work fills in some additional details of this picture of Reformation England. In his brief sketch, *Thomas Cromwell and the English Reformation*, Dickens argues that for too long credit has not been given to the role Cromwell played in the reform of Tudor England in the years from 1532 to 1540. Dickens shows that on some points More and Cromwell agreed, especially on the value of building up the state and of suppressing lawlessness, and that they were in fact, if not good friends, at least appreciative of each other's talents. What exactly was the achievement of Thomas Cromwell? I think that Professor Dickens would say that more than anyone else Cromwell was responsible for the successful implementation of the Royal Reformation. At the same time he pursued a policy that made the later Protestant Reformation not only theoretically possible but actually feasible and almost inevitable. He is thus the connecting link between the two even though he never lived to see the second.

The contrast of Cromwell with Lord Darcy, who was a leader in the Pilgrimage of Grace and who died for his crime, makes Darcy appear in one view as an attractive representative of a disappearing breed, yet a specimen that had to disappear as society changed. There was as little room for him in later England as there was for the "frontiersmen" in later American history. What could happen when these groups survived is amply illustrated in the wars of religion that ravaged much of Continental Europe. The ideals for which Cromwell labored preserved England from most of this. It is interesting that despite their disagreement with him, neither More nor Catherine of Aragon would ever join any such revolt. As Professor Dickens shows, Catherine may have been a proud Spaniard, but in this decision she was a true Queen of England and had perhaps a much better understanding of what this meant than her daughter Mary ever did.

Professor Dickens had begun with the Reformation in a major segment of England, the diocese of York, and then in the realm itself. He moved onto the wider field of the Continental Reformation with a short biography, *Martin Luther and the Reformation* and two major works: *Reformation and Society in Sixteenth-Century Europe* and *The Counter Reformation*.

His treatment of the spread of the Reformation follows the traditional lines but with some valuable additional observations. He notices the failure of the Colloquy at Marburg as sufficient proof that historians should be wary of too easily attributing purely political and economic causation or motivation to religious movements. It is obvious that these were not lacking at all times in the decisions of conversion or choosing of sides, but they do not always explain events. Professor Dickens is the first to admit that historians still do not have an adequate awareness of how these different strands fit together and are woven to create the scene as it actually happened. Too often religious events, political and economic events, and motives are simply presented as happening on different levels and hermetically sealed against contact and influence from one another. It must never be forgotten that this was still an age when men believed that disaster was a sign of God's displeasure and good fortune a sign of God's blessing. After all, had not Henry VIII, who was not a simple peasant but theologically astute, thought this way? The linking of these two levels was not invented by any sort of degenerate and self-satisfied Puritanism but is much older. Thus we must bear in mind, as Professor Dickens shows, that the very success of the reform movement on the political and social level was a theological argument in its favor. At the same time he points out that due attention has not been paid to the genuine missionary crusade manned by thousands of volunteers who went out from Wittenberg. This movement is often lost because of the stress placed upon the later and comparable missionary efforts arising out of Geneva

and the Counter Reformation. Another valuable comparison given by Professor Dickens is his portrayal of the spread of Lutheranism by means of traders, kings, and missionaries; one is reminded of a more famous trio in the New World.

In the area of social history Professor Dickens stresses well-known but still important insights. Historians can find no substantial connection between the secularization of Church lands and the depression of the peasantry in Lutheran areas, nor did the former lead to the expansion of the power of rulers and so provide the basis for later absolutist rulers. As he says elsewhere, in the earlier days the peasants, when dealing with the Church authorities and especially the monasteries, were facing landlords often no better and no worse than others. On the other hand the estates and the lords in most cases reaped whatever benefit there was from the confiscation of Church lands.

Professor Dickens attempts to give some answer to the question of the general impact of the Reformation on society in the sixteenth century. His basic thesis is that the Reformation must be seen as a climax and at the same time as another stage in a long period of tension and conflict as the whole basic structure was being questioned and tested on various levels. The Reformation was a major crisis in a series of crises, perhaps the greatest in the chain but by no means the last. It is clear that in this interpretation Professor Dickens is professing his adherence to that school of present-day historians who claim that unrest, change, and disturbance are far from being unusual in medieval and modern Western history; for them it is rather the periods that appear to be of a calm and almost static nature that require explanation. Dickens emphatically rejects the simplistic or single explanation; neither the Weberian, nor the Marxist, nor the sectarian explanations are enough.

The author makes some valuable suggestions for our understanding of the Reformation, its success, and its problems. For example, he links the development of printing and the number of new universities that were founded just prior to this era. He notes that the new mining ventures helped spread reform ideas from the Hussites in the fifteenth century, just as later the mines in the Tyrol helped the spread of Anabaptism, since in both cases a mobile population came into contact with these ideas and then moved on. He strongly urges that historians try to appreciate how powerful the forces of the irrational were in society at this time and what weight they must have had on the decision made by individuals in these circumstances of increasing tension caused by so many changes and repeated crises in the society. He gives a brief and excellent account of how complex the process of decision making was in German cities like Strasbourg, Nuremberg, and Augsburg, caught as they were between their business and professional interests and their religious convictions.

There are points where one might question a particular interpretation

or assertion. For example, Professor Dickens seems to say that the sixteenth century was marked by a "rising tendency" to intertwine Church and state (p. 80). Since he has also shown that it was a century marked by a growing secularism, this may seem a strange assertion. Again he portrays Germany as a frontier province (p. 23), while Poland and Bohemia are seen as part of Europe (p. 92), and this is an unusual switch. Still his basic assertions and insights remain strong and fruitful. He sees the major problem facing the Church in Switzerland on the eve of the Reformation as boredom rather than hostility to religion, but the era of Reformation and Counter Reformation, when men began to take strong stands on religious questions, was soon to change that. He shows that Lutheranism could never have succeeded in France because it was in conflict with certain basic institutions, while in other countries in Eastern Europe it was associated with foreign (German) domination. And so in both cases, if the Reformation was to come it had to be of another strain. Finally, he connects the radical Reformation to its medieval predecessors and background, and it is in this context that he is able to portray Anabaptism as the great scare of the sixteenth century. It was feared in inverse proportion to its actual threat; one example of violence justified all reprisals. It was to its time what anarchism and Bolshevism were to the earlier decades of our century, or perhaps what the New Left is to our own time.

From this picture of the Reformation Professor Dickens turns to the next stage, the Counter Reformation. From the start he admits that this is a study of a phenomenon that was at least a hybrid, for there were both Catholic Reformations and Counter Reformations, as has been well demonstrated by modern scholarship in this area. His intention is to present a sympathetic portrayal of a movement that for obvious reasons has never been well received in non-Catholic circles and is certainly out of favor among most contemporary studies by both Catholic and non-Catholic alike. In thus challenging the prevailing fashion Professor Dickens enables historians to attain an objective view of the men and the time. He also attempts to have us see these men as they saw themselves and as their contemporaries saw them. It is only fair to view the prodigious labors of Peter Canisius in the Empire as an outstanding feat, regardless of how people today think about those questions in which he was so able a disputant.

At the outset two propositions are quickly established in several chapters describing Christian society at the end of the fifteenth century. First, the Renaissance papacy with all its artistic patronage and its religious scandal was a natural result of the victory of the papacy over the councils in the previous century. Its victory over the councils meant simultaneously the defeat of all attempts to reform the Church. Thus in almost all the lands of Europe we find criticisms of the Church and its leaders; these attacks

come from both learned and popular circles. Professor Dickens confirms our impression that even prior to Luther men saw that the great obstacle to reform was the papacy itself and its curia.

The second proposition is that the Counter Reformation must be seen as a choice made by the Church between conflicting tendencies within her own body. It was not merely choosing against the claims and demands of the Lutheran, Calvinist, or Anabaptist reformers, but also choosing which groups to support from among the Catholic reformers. In the end it was the scholastic rather than the Biblical, the repressive rather than the humanistic traditions that won out. He argues that perhaps this was inevitable, for in times of war—and the image of the Church under siege was very much in the minds of these participants—only the hard-line, polemical approach seems to be safe. On this point, however, one might question the apparent assertion that the Council of Trent was conservative from the start (p. 114), for even the book shows clearly the conflicting trends and ideas.

The book abounds in useful insights. Great stress is often put upon the apparently fundamental divergence between the Catholic and non-Catholic—whether Lutheran or Reformed—doctrine of justification, yet time and again the major disputes were over the Sacrament of Christ's Body. His portrayal of Johann Eck is a sympathetic one; the man is seen not as a babbling and bumbling opponent who serves only as a foil for Luther, but as a skillful and sincere opponent, even if perhaps too pedantic and irascible for modern tastes. Dickens's account of the early Jesuits and their work presents much familiar material but adds two good observations. First of all, he notes how important their life in Paris was for Ignatius Loyola and the early members in that it freed them from being merely a reform movement associated solely with the Iberian peninsula (p. 75); second, he describes in a modern idiom a characteristic of these men, their "will to win" (p. 144), which perhaps goes far in explaining their success.

Professor Dickens does not restrict himself to or overemphasize the role of the Jesuits or of the other orders. He quite correctly sees that, with all their contributions, the religious orders could not solve the fundamental problem of the Church: the poor state of the lower clergy, a problem that was to plague reformers on all sides. There is a convincing picture of the growing ground swell of reform clearly presented in the program of 1537, demanded repeatedly at Trent, and painfully articulated and implemented over a period of decades thereafter. This movement was both progressive and reactionary since it brought to the cause men looking back and hoping to restore the old ways and laws as well as men who were looking ahead. The fundamental change that marked the victory of the spirit of the Counter Reformation is seen to be the decision to emphasize order, law, and discipline for the clergy, to stress the chain of command

as the most important tool for reform (pp. 101–02). As he notes, repression was to be linked with reform (p. 106), and Augustinian and Erasmian tolerance were discarded. The results of this policy are only too well known, but Professor Dickens does not end the book at this point although the title might suggest this. Rather, he goes on to the following generations and the next stage in the history of the Church. In this section he relies heavily on the school that makes a very clear distinction between the Counter Reformation and the Baroque (p. 165). The latter he aptly portrays as the result of a reform movement that has become part of the Establishment. To cite one example, he sees a clear contrast between the early Jesuits and their ideals and the Gesu Church in Rome built several decades later at the behest of aristocratic patrons. He sees this generation as marked by a neo-Renaissance spirit, and at the same time he differentiates with great care between what the Renaissance papacy had done for Rome and the really modern concept of urban renewal one finds in Pope Sixtus V (p. 143).

These later decades are marked, too, in the political world by the shifting and maneuvering of the papacy to escape total dependence upon Spanish and Habsburg power. Rome constantly strove for a balance of Catholic powers. The Wars of Religion are seen in this focus, for it always seemed that no matter who won, the papacy lost. In the French Wars of Religion the shrewdness of Sixtus V is seen in that he opted for Henry of Navarre rather than the Catholic League. Professor Dickens shows that in the seventeenth century the cultural and intellectual exchange continued between sections of divided Europe but that, as time went past, the flow of cultural influence became one-directional—a sign of the hardening and closed nature on the Catholic side. The states maintaining this attitude tended to become backwaters in European life and in economic and social development.

Professor Dickens has thus traveled in his researches from the late Middle Ages in northern England to Europe on the eve of the Cartesian revolution. He readily acknowledges his debt to innumerable scholars from whose research and monographs he has drawn a wealth of material for these books. Yet the corpus of his own writings is not simply a neat summary of contemporary writing on the Reformation era. There is an amazing grasp of detail, of the complexity of both events, and of the motivation of the men who created the events or were involved in them. There is a consistent and sympathetic awareness of and openness to the many men he studies. As a historian he wishes to see them as they were, in so far as this is possible, and not just as their friends and opponents have portrayed them, but he is also aware of how recent generations have questioned these men. He has a remarkable gift of description by which he often captures a man, a movement, or an event in a capsule phrase.

In summation then, Professor Dickens would remind historians not to

forget the little people. Decrees do not change a society—the reform decrees of the late Middle Ages bear witness to this fact—and it is mostly the man in the street who must carry out the decrees if there is to be reform. Dickens would ask us all to remember that when we study the great movements of the sixteenth century, we should not consider merely the events or only the great men who initiated them or carried them on; we should also consider those who were never given the chance to be heroic and who were very thankful for avoiding this mixed blessing. As he concluded his study of England in this period, he observed that religious controversy was not often something that reached down into the lives and hearts of most men, and perhaps this is a reflection on their religious commitment; but then again in the lands where the conflict was felt more widely one finds the fierce and bloody “religious wars” of France, the Netherlands, and Germany. Heroic ages and choices have always been rough on the ordinary man, and it is his world that Professor Dickens has striven to portray. He has succeeded in so doing in a remarkable fashion.

The Partition of India: A Quarter Century After

A Review Article by ROBERT ERIC FRYKENBERG

NICHOLAS MANSERGH, editor-in-chief. E. W. R. LUMBY, assistant editor. *The Transfer of Power 1942-7*. Volume 1, *The Cripps Mission, January-April 1942*. (Constitutional Relations between Britain and India.) London: H. M. Stationery Office; distrib. by Pendragon House. Redwood City, Calif. 1970. Pp. lxiii, 928. \$25.20 postpaid.

H. V. HODSON. *The Great Divide: Britain—India—Pakistan*. New York: Atheneum. 1971. Pp. xii, 563. \$12.50.

C. H. PHILIPS and MARY DOREEN WAINWRIGHT, editors. *The Partition of India: Policies and Perspectives, 1935-1947*. Cambridge, Mass.: M.I.T. Press, Massachusetts Institute of Technology. 1970. Pp. 607. \$15.00.

NEARLY A QUARTER CENTURY has passed since the Indian Empire came to an end. The enormous consequences of that event and its serious consequences for global peace are difficult to exaggerate. What occurred in 1947 was a profound fracturing in human relationships, a smashing of the structured system of unity and stability by which many peoples within the basin of the Indian Ocean, if not also those in adjacent mountain ranges, had known a high and growing degree of tranquillity, order, and security. *Pax Indica* was shattered. It was shattered in such a way that it may not be put together again, certainly not for a long time.

Before examining this event closely, let us glance back down the corridor of subsequent events. As of this writing, guns have ceased firing in Bengal and the tide of refugees, which had reached nearly ten million and had carried torrents of incalculable human misery, has begun to recede. The year 1971 marked the emergence of Soviet naval power in the Indian Ocean, with many ominous implications. It also marked the near end of East of Suez positions and abandonment of the Persian Gulf by Britain, both legacies of Indian imperial power, insupportable without such power. These withdrawals threaten to leave peoples from India in Africa and Asia further exposed and undefended. Since 1969 increasing clouds of communal violence have hung over Indians in Malaysia and Singapore. In 1968, after years of growing tension, many Indians in East African countries

were put to flight. A seven day war in 1967 closed the Suez Canal, once the gateway of Indian imperial power to Africa and the West. The late fifties and early sixties brought a rising incidence of anti-Indian (anti-Tamil) political behavior in Ceylon. The same period saw most remaining Indians driven out of Burma, completing a series of expulsions that had begun with the Saya San events in 1932 and with repressions coincident to the Japanese invasion in 1942. The 1950s saw steady streams of refugees coming into India from Tibet. None of these processes was necessary or irreversible. In each case Indian imperial power diminished, with consequent hardship and suffering for peoples of India.

Yet all of these events are also measured as trifling beside the holocaust of the Partition. This great divide broke in sunder a gigantic and highly complex political (if not also economic and social) system that had evolved over nearly two centuries. Other events surround this central event and fly from it as sparks from a crumbling wheel. Without that Great Wheel (ancient symbol of *Chakravarttha*), smaller fragments of human organization in the area no longer dwell with the same measure of security and peace. Beyond this huge geopolitical low-pressure area of great strategic significance the two giants of global power brood fitfully and stir uneasily over each outbreak of trouble.

It is in light of such circumstances that all three books being examined here have special significance, each for different reasons and each from different angles of vision.

On June 30, 1967, Prime Minister Harold Wilson announced that a new series of *Documents on the Transfer of Power in India 1942-7* would be published. To be edited by "independent historians who would be given unrestricted access to the records and freedom to select and edit documents for publication," this series—which will probably comprise ten to twelve volumes when completed—would cover events concerning constitutional relations between Britain and India from the Cripps Mission to the actual transfer of power on August 15, 1947. The first volume, edited by Professor Nicholas Mansergh and Mr. E. W. R. Lumby, came out late in 1970. It centers on the Cripps Mission and, in 928 pages, only covers January to April 1942. The volume consists of reports and correspondence between governors in British India and the governor-general (or viceroy); telegraphic correspondence between the viceroy and the secretary of state for India, weekly letters exchanged between them; various sets of official minutes, both of the India Office and of the War Cabinet Committee on India; and papers or memoranda circulated, including some drawn from the prime minister's office that record notable exchanges with heads of state, in the United States and in the Dominions. Most of this material has never been available to the public before nor would it have been under the new thirty-year rule.

The significance of these documents, of course, lies in the inside view they afford. We can now study decision making in detail and learn how

men brought about the ultimate dissolution of the Indian Empire; or, from another perspective, how men achieved independence for India and Pakistan. We can scrutinize the working out of historic processes. These records throw new light on the outlooks and responses of leadership, both Indian and British, to challenges from highly complex and rapidly changing situations. They show us how personalities shaped events. One sees in high places of authority and influence individuals potentially capable of wise counsel and decisive action in the restoration of good faith who are equally prone to flounder in webs of private frailty or nets of group pressure. With some horror one can see the little games men play while, at the same time, as with slow-motion cinema or the steady ticking of a time bomb, none seems to perceive the dimensions of the human tragedy that lies in wait.

Looking back from our own vantage point, two processes, working in coordination like gears meshing in one smooth motion, appear vitally important. First, without that relatively steady and continuous process of socio-political and socioeconomic integration, consolidation, and expansion, which had begun centuries earlier and had gained speed in the eighteenth and nineteenth centuries; without that ever growing balance and contrived partnership or harmony between avowedly distinct classes, communities, castes, and creeds; without what since the early nineteenth century had been called "Indianization" no amount of indigenous participation in self-government, whether autocratic or democratic, would suffice to hold such a huge political system together—at least, not for long. One should not forget that, within a fantastically differentiated and segmented socio-political structure, where fragmentation and localizing tradition were virtually normative, political systems of great size and strength in India had been the historical exceptions. As such they had required exceptional diligence and adroitness in the delicate balancing of social forces. "Indianization" could not be taken for granted.

Second, from the earliest years of Company activity in India, local participation in government had been recognized as essential and inescapable, as the very key to power. A mere handful of Britons had never, nor could they have ever, really governed millions. Improvement in the forms of "Native" rule, indeed of self-rule, had been the constant preoccupation of successive statesmen since the days of Hastings and Cornwallis, of Munro and Elphinstone, of Bentinck, Macaulay, and Trevelyan. With gradual transition from autocratic and despotic to more representative and democratic institutions, from long before the viceroyalty of Lord Ripon, the logic of devolution to self-rule had been inescapable. In the 1880s municipalities, districts, and provincial governments had become testing grounds for local self-rule. By successive stages, culminating in dyarchy after 1919 and in virtually the whole responsibility for governance after 1935, this course had been followed, albeit with varying success and at uneven speeds.

The problem of the twentieth century was the growing failure of these

two processes to synchronize. Demands for greater roles in self-government and political movements generated to force the pace of such participation began increasingly to outrun the pace of the broader "Indianization." As a result the widening gap between these two processes brought increasing social tension and communal conflict. Indeed, in some respects, the larger prerequisite process was reversed; sociopolitical distance between important communal elements began to increase. One has but to scan a quantitative chart giving the mounting incidence of communal violence, especially in the 1920s and thirties, to see how this happened. And during the decade from 1937 to 1947, particularly in the final five years, Indians were to see the bitter culmination and consequence of these dysfunctional processes, inexorably leading to catastrophe. Self-rule and independence without communal integration and harmony could only lead to breakdown and no-rule, at least within the wider context of the subcontinent as a whole. *Pax Indica*, in the larger sense, collapsed, and a state of war took its place. This persists to this day.

The essence of the problem was summarized succinctly by the India Office on January 28, 1942. "The political deadlock in India today is concerned, ostensibly, with the transfer of power from British to Indian hands. In reality it is mainly concerned with the far more difficult issue of what Indian hands, what Indian Government or Governments, are capable of taking over without bringing about general anarchy and even civil war. The former issue has been settled *in principle* [my italics] by pledge after pledge. . . . What would be equally necessary in the application of that principle in India . . . is some measure of agreement as to who constitute the people or peoples whose freedom of choice as to their form of government is to be respected" (p. 81). In essence the question was one of rightful succession.

However perceptive such a realization might have been, the fact remains that, between 1937 and 1942, the British failed to produce answers or to provide the political initiative. This is shown in Lord Linlithgow's opinion that "India and Burma [had] no natural association with the Empire, from which they are alien by race, history and religion, and for which neither of them have any natural affection, and both are in the Empire because they are conquered countries which had been brought there by force, kept there by our controls, and which hitherto it has suited to remain under our protection" (p. 49). An outraged Clement Attlee castigated the "crude imperialism" of the viceroy adding that, if such were the case, it "would form the greatest possible condemnation of [British] rule in India and would amply justify the action of every extremist in India. . . . All India was not the fruits of conquest; large parts of it had come under [British] rule to escape tyranny and anarchy. The history of at least 150 years [had] forged close links between India and the United Kingdom" (p. 11).

At the insistence of Attlee, Stafford Cripps, an authority who knew Congress leaders and who was committed to the cause of Indian freedom, was sent. The Draft Declaration he carried recognized India's place as a full "free and equal" partner in a "fellowship of independent and freedom-loving nations." "The constitutional structure [was] to be of Indian devising and to correspond to Indian social, economic, and political conceptions, and to the peculiar conditions of her complex structure. . . . In spite of all the wide divergencies of her internal structure . . . an underlying unity of interest and of outlook in India [was] worth preserving."

We sincerely trust that Indians will be able to find agreement upon some constitutional scheme which . . . will preserve and establish on firmer foundations that essential unity of India as a whole which the course of history has shown to be of such vital importance both to her internal peace and prosperity and to her security and dignity in relation to the outside world (p. 258).

"As for procedure," that also was to "be decided by agreement among Indians themselves" (pp. 256-66).

The tragedy of the Cripps Mission lies in miscalculations and tactical blunders. Cripps allowed personal sentiment to betray him. Revealing at the outset his whole offer—immediate creation of a new central government, with major party representation and major responsibility, and full independence for a union of India at the close of the war, in return for Indian war support—he trapped himself. Gandhi and the Congress mistakenly saw British panic and collapse. Amid turmoil, chaos, and recoiling defeat before Japanese invaders, Congress leaders fancied immediate and total independence within their grasp. In his first interview, after denying that he spoke for the Congress, Gandhi doubted whether Congress would accept. He objected that no provision had been made for democratizing the princely states. He felt that choice of accession or nonaccession to the union should not be allowed and that control of Defense should not remain in British hands. Cripps, having made all his concessions, found no retreat. He could offer no more. Even so, London worried that he might have given away too much.

Congress leaders were later to regret their own shortsightedness.¹ Gandhi acknowledged that Jinnah's influence and the Pakistan movement had grown tremendously since the Lahore Resolution of 1940. He admitted that the British sincerely wished to leave a strong, united India. But, as the viceroy had predicted, Gandhi saw the Cripps Declaration as an open invitation for Muslims to create a Pakistan. Gandhi's words show unwillingness to come to any agreement. Cripps should not have come to India, with a "cut and dried scheme to impose on Indians" (p. 499). Time and again Gandhi begged Cripps not to publish the declaration, for publication would put pressure on Congress leaders and deprive them "of an opportunity to bar-

¹ See C. H. Philips, "The Partition of India," *The Twenty-fourth Montagu Burton Lecture* (Leeds, 1967), 24.

gain for a better position" (p. 500); the offer was "a post-dated cheque on a failing bank." Too late it was realized that no other scheme would come before the end of the war and that agreement between Hindus and Muslims was prerequisite to final settlement. Gandhi himself began to foresee ominous shadows.

The documents reveal other fascinating sidelights. Europeans worried about "special rights for British subjects" and security for business interests and properties in India. They learned that unless they accepted Indian nationality they would become foreigners subject to treaty arrangements. Ambedkar and Rajah, speaking for the Depressed Classes, saw the proposals as "calculated to do the greatest harm" and "sure to place them under an unmitigated system of Hindu rule." Anything that "went back to the black days of the ancient past [would] never be tolerated." Speaking of determination "to resist such a catastrophe" they begged the British not to break faith with millions of backward people in India (p. 603). The Hindu Mahasabha, led by Savarkar, lectured Cripps on majority rule. Opposing any "right of non-accession" and expressing distrust of Muslims it rejected the offer (p. 627). The Sikh delegation, led by Baldev Singh, told Cripps that all hopes for due consideration as an important community were lost and that any separation of Punjab ("our motherland") would be fiercely resisted (p. 586). Even the Justice party of Madras spoke of secession from the union so as to avert Brahmin domination (p. 555).

THE IMPORTANCE OF H. V. Hodson's *The Great Divide: Britain—India—Pakistan* lies in the vantage point of its author, the freshness of its sources, and the quality of its analysis. Deeply involved in the great events and large questions leading up to the Partition, the author personally knew the key figures—Gandhi, Jinnah, Nehru, Linlithgow, Cripps, Wavell, Mountbatten—and hosts of other leaders and officials, Indian and British. Hodson has brought the special insight of personal experience to his analysis. His work, which bears the fruit of years spent in research, reflection, and writing, was begun in 1963, at the urging of General Lord Ismay and M. V. P. Menon, both friends and fellow participants in the events who have since died, and of Lord Mountbatten, last viceroy of India.

Lord Mountbatten's contribution gives this volume special significance and originality. Hodson was entrusted with papers Mountbatten had brought back from India, and Mountbatten encouraged Hodson's research, patiently and frankly answered questions, shared recollections, and, finally, read the manuscript and offered critical comments. While the author has provided us with a massive and balanced piece of polished historical craftsmanship, the volume also dimly reflects, as "through a glass darkly," some of the power and personality of Mountbatten himself. Even to the special permissions granted by Her Majesty, Queen Elizabeth II, and by the

governments of India and of Pakistan, the mark of Mountbatten lies in the shadow nearby.

Once more the reader pauses to ponder the fateful early months of 1942. Those who lived in India at the time will recall scratchy "wireless" voices from Japan calling all Indians to revolt, attempts to prepare defenses and to give aid to Burma refugees, and the general calmness of the country. All went on as before—politics included. As he was constitutional adviser to the viceroy, Hodson is able to give us an insider's view of the Cripps episode, to show us that it was ill-conceived and foredoomed. The scheme itself was a compromise formula for preventing a split in the British War Cabinet. This, in turn, brought about a fundamentally mistaken mission, the sending of an emissary to India without prior Indian agreement. The plan would have been unworkable without Indian support (both official and national). Local confidence was further ruptured by another scheme devised by Cripps and Roosevelt's envoy Johnson, to put Congress on higher ground. That most of "the talks had been carried on over the heads of the Muslims and other parties," as Jinnah complained, seems largely true (p. 102). Yet, when Cripps saw the letter dated April 9 from Maulana Abul Kalam Azad, Congress president, who demanded a "National Government responsible to no one but itself" and constituting "an absolute dictatorship of the majority," Cripps knew he had failed (p. 102). He left India. A "Quit India" campaign soon began. Arrests of Congress leaders followed. Insurrection occurred, mainly in Bihar and the Eastern U.P. (United Provinces). Within three weeks government control returned. The failure of insurrection showed how strong the existing order remained. Masses of people remained indifferent, as did most minorities and the bulk of official, professional, and commercial classes. "Thenceforward, for five years to its end, the power of the British raj was never challenged on a grand scale. The presumption is that but for its own will . . . it could have continued for a long while longer. But the will to stay had gone, and after the Cripps Mission it could not be revived" (p. 107).

EVEN AFTER a quarter century those who went through the terrible months of 1947 find it difficult to look back on the Partition with detachment. Yet some who played significant roles or who had a hand in shaping events also held unique vantage points from which to view what went on. Their personal testimonies may afford future generations means for better understanding of those events that so radically altered the history of so many peoples. The task of collecting evidence from those still alive, asking some to set down their own parts of the record, is a great service. It has been well carried out by C. H. Philips and M. D. Wainwright—respectively director and research officer of the School of Oriental and African Studies, University of London—in their invaluable contribution to historical scholarship, *The*

Partition of India: Policies and Perspectives, 1935-1947. The book grew out of a series of seminar sessions, spread over three years, between “participants” and historians. A priceless collection of evidence and comment was brought together in London. Papers of the All-India Muslim League were rescued. New studies were generated. Official documents in Britain were published.

Even so, this volume does not pretend to be comprehensive. Rather it attempts to present more evidence, more analysis, and to ask more questions. It suggests new investigation and further study. Two broad categories of contributors correspond to the two sections, one on “Policies and Parties” and another on “Perspectives and Reflections.” The first is based largely on academic study of records; the second comes from personal experience and observation (Pakistani, Indian, and British). Many persons (some forty or more) while they did not publish papers in this volume, contributed indirectly, going to much trouble, giving time, and traveling across the world.

Some of these essays are cold and clear, others are polemical and blurred. A high standard of excellence, both in analysis and detachment, is set by the introduction. Superbly crafted and provocative, it stands as a model for the others to follow.

One of the best essays is by C. S. Venkatachar, who served in the Indian civil service from 1922 to 1961. From 1933 to 1937 a district officer in Lucknow, and from 1941 to 1946 commissioner of Allahabad, at the Partition he was prime minister (*diwan*) of Jodhpur. What he has modestly called “some personal impressions” constitute a tough-minded, insightful, and analytical contribution. With skill and authority he shows how choices became fewer and fewer. The British were bound by their past. The Congress was caught by its opinions. The Muslim League balanced a war of nerves.

Himself a Hindu and a nationalist, Venkatachar numbers Congress failures with ruthless efficiency. It refused to form a coalition with the League in 1937, attempting instead to win Muslim masses over the heads of their leading classes and allowing itself to become party to an attack on Muslim culture at its very heart—in language and curriculum. Abandoning positions of strength in provincial power Congress forced itself upon the Center. British offers of conciliation and certain power in 1942 were rejected by the Congress, while its policies vacillated between Gandhi and Nehru. It launched an abortive “Quit India” insurrection that brought inevitable incarceration and left a clear field to the League. And, finally, it balked and fretted its final options away until only tragedy could remain. In these ways the genesis of Pakistan and of Partition became certain, even in 1942, and steadily more certain thereafter. “Congress was fantastically unreal in its estimate that it had the strength to envelop the Muslim majority areas.”

Of the British, Venkatachar can only say that their best work lay behind them; that “Linlithwaite” rule was barren and bankrupt; and that “wise instinct” dimly told them how, in a choice between force and conscience, their day was over. But, like Congress, Britain failed India’s peoples, having no more will or strength to forge unity. Stepping stage by stage away from power, “they found themselves bereft of influence.”

As for Jinnah and Pakistan, few truly realized how much of “a revolution within a revolution” had been going on. This was the product of fear and hatred—exasperating postures of arrogance and hurt pride, of “Islam in danger,” of sinister emotions. Proud Jinnah, taunted and weeping, often abandoned, was to have his revenge. Congress, making its “biggest mistake,” underrated him. It clung to pretensions that it could hold the Muslim leadership, failing to realize what a bane its Muslim “leaders” were and how it was blinded by its illusions (p. 484).

In the final reckoning, Venkatachar reminds us that

the British were not dealing with politicians playing a parliamentary game of politics . . . but with the charismatic leaders of India and Islamic versions of western nationalism, worshippers of the Moloch of the modern nation state. With all their modern culture, their noble characters, their intellectual integrity, their strikingly high degree of sophistication as western elites, Jawaharlal Nehru and Muhammad Ali Jinnah had not fully assimilated the alien knowledge; they wandered between the two worlds and belonged to neither. [Therefore,] it is not a profitable endeavor to distribute proportionate shares of blame between Nehru, Jinnah, and the British. None of them were in a position to find a way out of a revolutionary situation. All revolutionary changes exact their price (p. 489).

In India the price was paid in blood and fire, by countless ordinary people.

What then do these new publications show us? First, probably the last politically astute and potentially creative act of the Indian Empire was the Government of India Act of 1935. From that time onward, down to the last desperate actions of Lord Mountbatten (“right man at the wrong time”), too much clumsiness, ineptitude, or insensitivity held sway in the face of mounting violence. Second, the transfer of power began with the 1935 act or, more correctly, with its implementation of provincial autonomy in 1937. After that, events rapidly overtook timetables. Third, in the progression of events from 1937 to 1947 there are elements that might be described as classical tragedy. All the major parties were noble, heroic, and right—not merely by their own lights but, granting their assumptions, by common human standards. Yet each, moved by distrust, fear, and sense of mission, looked for danger in the wrong direction, refused to heed warning signals, and turned toward a calamity that no one wanted. The Congress feared the British more than the League; the League feared the Congress more than the British; the British fretted over the League (and other threats to union, such as the princes and minorities) instead of heeding the Congress.

Finally, greater distance and detachment, with depositions and documents, dramatize clearly the immensity of human tragedy. However culpability may be assigned—and it seems evenly shared—few deny that it was a tragedy. How avoidable it was will be argued by future generations. Whether the tragedy is yet over is also arguable. Quite apparently the price is not all paid. Indeed, the partition of Pakistan that brings such suffering to people of Bangla Desh leads one to argue that Pakistan has lost its mandate for existence and that restoration of political unity for the whole subcontinent might yet be the best solution. But the “how” remains unanswered.

Reviews of Books

GENERAL

ANNE G. WARD *et al.* *The Quest for Theseus*. With a preface by REYNOLD HIGGINS. New York: Praeger Publishers. 1970. Pp. 281. \$13.50.

As an example of current styles in bookmaking this is a gem. Anne Ward has assembled a number of able scholars to explore the myth of Theseus over the centuries: Ruth Edwards for the classical picture, W. Robert Connor for the historical development of the myth at Athens, and Simon Tidworth for literary and artistic references from the Romans to Picasso's Minotaur. Anne Ward herself contributes an introduction suggesting that we shall chase the "real" Theseus, and she also has written several chapters on Bronze Age Crete and Greece. Her comments on bulls and Cretan palaces are scarcely novel and do not tie down Theseus to a specific period as neatly as she suggests; it is rather surprising that the text does not cite the possible occurrence of the name of Theseus in Linear B.

In themselves the several essays contribute nothing of note to our knowledge; Tidworth, in particular, has so large an area to cover that his sections are interesting but almost haphazard catalogs. One might regret so much expenditure of ink and paper, for the tale of Theseus is a clear example of the adaptation and amplification of myth in historic times for cultural purposes. The serious student of this development will turn to Hans Herter's fundamental studies or den Boer's lecture, both of which are listed in the bibliography of this work.

What makes the book fascinating to the present reviewer is its ample supply of black-and-white and colored illustrations. True, they sometimes have no connection whatever with

Theseus, and the captions (added by the publisher) are not always accurate; but they are very pretty, and well placed on the pages. One keeps waiting to see what will appear next; rarely has so much been made out of so little straw. Still, a general lover of Greece will not be misled by the text and will admire the handsome illustrations.

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GÜNTHER MOLTSMANN and KARL FRIEDRICH REIMERS, editors. *Zeitgeschichte im Film- und Tondokument: 17 historische, pädagogische und sozialwissenschaftliche Beiträge*. Göttingen: Musterschmidt-Verlag. 1970. Pp. 337. DM 33.

These seventeen essays provide a scholarly, stimulating, and wide-ranging introduction to an important subject: the potential of, and the limitations on, the use of audio-visual materials in historical research and teaching. Concerned as much with underlying theory and with methodological approaches as with concrete examples of work done, the essays are grouped around four themes: audio-visual "documents" as historical source material; the history and social function of various media; experiences using such materials in the teaching of history; and the holdings and activities of German and Austrian audio-visual archives. Television and photography receive relatively short shrift. Radio receives more attention, but the emphasis is on film.

One of the dangers of using media as historical source material is illustrated in Gerd Albrecht's essay. Author of a very stimulating book applying quantitative analysis to commercial films in Nazi Germany, Albrecht elaborates

a thorough and medium-oriented methodology and then analyzes a twenty-minute UFA newsreel of the military parade celebrating Hitler's fiftieth birthday in April 1939. He concludes that the army, not Hitler, is the star of the film, although he indicates a variety of film techniques that underline the army's subordination to Hitler. This emphasis on the military over the political leads Albrecht to assert that it marked a turning point in Nazi propaganda and the preparation of the German population for the attack on Poland. But no direct link between the makers of the film and the Nazi leadership is established. And the conclusion simply does not take into consideration the medium. A crowd and official observers, important politically or not, are static. A parade moves constantly, offering new faces, new objects, and new spatial relationships, which maintain audience attention far more easily than crowd shots to which drama is lent solely through editing and commentary. The medium of print is quite different, as Albrecht himself establishes, but a film maker, interested solely and simply in making a "good" film of such an event, will inevitably give greater coverage to the parade.

The various authors of these essays agree on the difficulties, but they agree as well on the necessity and the potential fruitfulness of using audio-visual materials. As Moltmann points out, to understand the nature of the Nazi regime, which only greatly exaggerated public and theatrical aspects common to all modern states, it is as important to study the self-image the regime sought to propagate as it is to study what actually existed. Karl Arndt, exploring what film can bring to the architectural historian, concludes that not only has it recorded many structures that have since disappeared, but that film alone can communicate the public function of a structure and the role it played as a single element of a larger design. Will Lütgert examines a commercial film of 1941 for what the Nazis projected as acceptable patterns of behavior and the means exercised in youth groups to reach these goals. Ernst Opgenoordth communicates exciting results of university students themselves experiencing the difficulties and rewards of extracting meaning from film.

The essays are extensively footnoted, and the

footnotes frequently lead the reader beyond the material specifically covered. The bibliography and the index are quite good. An appendix describes the structures, aims, and functions of eight German centers of study concerned with the historical analysis and pedagogical use of audio-visual materials.

The picture that emerges from these essays of the use made of such materials in Germany is indeed an encouraging one: interinstitutional cooperation, reasonable financial support, interdisciplinary stimulation, an increasingly sound theoretical foundation, a growing recognition of the unique testimony that audio-visual documents can bring, and the development of a variety of methodological approaches designed specifically for such material. The book merits translation, and the German example imitation.

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P. N. POSPELOV. *Problemy istorii (Stat'i i rechi)* [Problems of History (Articles and Speeches)]. (Akademiia Nauk SSSR, Otdelenie Istorii.) Moscow: Izdatel'stvo "Nauka." 1968. Pp. 207.

This selection from the published articles and speeches of P. N. Pospelov appeared on the occasion of the seventieth birthday of their author. A member of the Communist party since 1916, of its Central Committee since 1939, of the secretariat of the Central Committee in 1953-60, and a candidate member of its ruling presidium in 1957-61, Pospelov has been a party-worker-historian in charge of the ideological "front" since the late 1930s. Like his senior in the party apparatus, M. A. Suslov, he held critically important positions from the point of view of opinion shaping in the Soviet Union: for many years, he was on the editorial board of the theoretical organ of the party, *Bol'shevik*, editor-in-chief of *Pravda*, and director of the Central Committee's Institute of Marxism-Leninism. A member of the Soviet Academy of Sciences and its presidium, he directed, more recently, the work of the editorial commissions in charge of publishing an official biography of Lenin, a six-volume *History of the Great Patriotic War of the Soviet Union 1941-1945* (1960-65) and a multivolume *History of the Communist Party of the Soviet*

Union (1964-). According to the reminiscences attributed to Khrushchev, Pospelov was also the head of the commission charged with investigating Stalin's misdeeds; if this allegation were true, then one of the most thoroughgoing dogmatists of the Stalinist era would be a main contributor to Khrushchev's famous "secret speech" of February 1956 initiating the process of de-Stalinization.

Be this as it may, the collection of articles and speeches under review contains little evidence to undermine the author's impeccable credentials as a hard-line Stalinist. Stalinism without Stalin is the spirit that permeates the book, which, characteristically, opens with a Lenin Day address given in January 1951. The speech, republished under the telling title "Lenin about the American Intervention against the Soviet Republic in 1918-1920," marked, in C. E. Black's words, "the climax of the anti-American campaign in Soviet intervention literature" (compare *Rewriting Russian History* [2d ed., 1962], 370-71). Claiming that "the intervention of American imperialists in Russian affairs, and their efforts to subdue the Russian revolution in order to completely subjugate Russia and freely rob her of her natural resources had begun even before October 1917" (p. 8), the speech blames the "American imperialists" whose "hands are dripping with the blood of Russian people" (p. 12) for plans aimed at the partitioning of Russia and describes Woodrow Wilson as one of the chief inspirers of the armed intervention against the Soviet Union. Pospelov quotes Lenin, who compared Wilson to Nicholas I, the suppressor of the Hungarian revolution of 1848, and accuses the "agents of Wilson" of having been gendarmes quelling popular revolutions in Europe and organizing hunger blockades: he does not fail to draw the appropriate conclusions from the lessons of history as interpreted by Lenin when referring to the "ruling circles of the U.S.A. who, aspiring for the domination of the world are pushing the American people with furious speed into the abyss of a new third world war!" (p. 14). Another Lenin Day speech made by Pospelov in 1954 and containing no anti-American invectives in regard to the intervention period (Black, p. 379) was not included in the booklet, subsequent parts of which deal, in chronological order, with the

December uprising of the workers of Moscow in the Russian Revolution of 1905, the importance of Lenin's ideas in fulfilling the task of building communism, the historic significance of the Second Congress of the Russian Social Democratic Workers' party, the centennial of the foundation of the First International, the results and lessons of the "Great Patriotic War," the connection between the Seventh Congress of the Comintern and the Popular Front in France, the fraternal relations between the Soviet Communist party and the (East) German Socialist Unity party, and in the concluding fifth of the work, once more, with the role of Leninism in the building of communism in the Soviet Union.

The sections just listed contain no new information for the student of history although shifts in emphasis may be of occasional interest in this flow of repetitive and dreary propaganda. He will take little comfort in the circumstance that the centennial celebration of the triumph of the ideas of the international association of workers was used by Pospelov for an attack on Mao Tse-tung and "the anti-Soviet nationalistic policy of Chinese leaders" (p. 87), who were compared to ultra-Left Trotskyites and dubbed anti-Marxist and anti-Soviet opportunists on other occasions (pp. 68, 174, 187-88, 205). All this is but a variation of the old song so familiar from Stalin's days and revived with reference to a citation from a speech by Hungarian party leader János Kádár: accordingly, the criterion of being a communist, a true internationalist, and a sincere defender of peace is solidarity with the Soviet Union, and he who slanders the Soviet Union cannot be an internationalist (p. 85). Likewise well known is the other recurrent theme of the collection, which paraphrases the anti-American vituperation so evident in the opening section of the work. As a regrettable example, one might mention the simplistic interpretation of the background of World War II and the clumsy effort to prove that the Soviet victory over the German war machine was attributable to the advantages of socialist economic organization, the logical conclusion being that any new aggression against the "camp of socialism" would inescapably lead to the destruction of the entire system of capitalism (pp. 120-21).

The ominous new confirmation of the thesis once presented in the report of Georgii Malenkov, then secretary of the Central Committee, to the Nineteenth Party Congress in 1952 is the more unfortunate since shortly after the last party congress attended by Stalin, Malenkov admitted during his brief tenure as prime minister that a nuclear war would be a disaster for all mankind and not only the so-called capitalist world. In fact, Pospelov himself is well aware of the implications of thermonuclear weaponry (pp. 117-18) and even uses this as an argument when he wishes to castigate the "criminally adventurous and frivolous attitude" of Chinese leaders toward the fate of humanity (p. 67). Thus, throughout the volume, he shows himself as a political agitator rather than a scholarly historian trying to prove that the Soviet Union's military and economic might enabled the development of the struggle for national liberation all over the world, prevented the imperialists from interfering with the domestic affairs of the people's democracies, and unselfishly promoted the building of socialism in all fraternal countries and the cause of peace on the international scene (pp. 97, 202-03). One may point out that the book was sent to press at the end of May 1968, that is, less than three months before the invasion of Czechoslovakia by Soviet troops. Yet even earlier, ever since the Soviet Union had become a great power in her own right, a critical eye could detect serious discrepancies between Marxist-Leninist slogans and Soviet propaganda on the one hand and historical facts on the other. *Problems of History* makes no such distinctions; if it is to serve as a guideline for Soviet historiography, serious Soviet historians—and we too—will have to face many more problems. Indeed the work exudes a degree of indoctrination insulting to the informed intellectual, let alone the trained historian, which makes it difficult to use rational arguments beyond pointing out crude errors or arrogant distortions of history.

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GERHARD PFEIFFER, editor. *Nürnberg: Geschichte einer europäischen Stadt*. [Munich:] Verlag C. H. Beck. 1971. Pp. xxiv, 619. DM 75.

GERHARD PFEIFFER, editor, with the assistance of WILHELM SCHWEMMER. *Geschichte Nürnbergs in*

Bilddokumenten. [Munich:] Verlag C. H. Beck. 1970. Pp. 120, 359 plates. DM 48.

Gerhard Pfeiffer, professor of history at the University of Erlangen-Nuremberg, has edited two volumes on the history of Nuremberg that represent local history at its best. With a number of able specialists he produced a history of the city as a political, economic, and cultural center, presenting a new evaluation of its traditions and relating them to the present. This effort, supported by the city council's committee on "The Albrecht Dürer Year 1971," was begun in 1968 and completed in time for the five hundredth anniversary of the artist's birth for the purpose of showing the city's role in local, German, European, and world affairs during its various historical epochs.

A novel feature of this undertaking was the publication of a separate volume of pictorial documents illustrating the city's development. With the assistance of Wilhelm Schwemmer, director of the Municipal Art Collections, the editor selected 359 illustrations reproduced on 220 plates, including seventeen in color on ten plates. They begin with a picture of an embossed gold foil cone from about 1200 B.C. and conclude with an aerial photograph of Langwasser, a modern housing development for sixty thousand of the city's inhabitants. They are presented under the following headings: Nuremberg as a royal city, rivalry with her neighbors, council and community, cultural life, life of the common people, the parish churches, monasteries and hospitals, memorials, art prior to Dürer, Albrecht Dürer, art to 1806, intellectual life, trade, crafts and industry, the age of liberalism, political destiny, and the city rebuilt.

Schwemmer provided the illustrations with appropriate captions and with lengthier descriptions at the beginning of the book. These descriptions were translated into English and French. The book contains an index of illustrations and another of their sources. In both, the illustrations are referred to by their numbers. Their value is enhanced by the fact that the volume on the history of Nuremberg has references to them in its margins.

Completion of the historical volume within three years was made possible by Pfeiffer's assignment of topics of limited length to thirty-eight specialists. He grouped these topics into the following broad divisions: prehistory, de-

velopment of Nuremberg as an imperial city (1050–1347), the first flowering under the Luxemburg kings (1347–1437), the city's great era (1438–1555), the city from the Peace of Augsburg to the Peace of Westphalia (1555–1648–50), decline and end of its autonomy (1650–1806), the city under the Wittelsbachs (1806–1918), and the city in the state of Bavaria (1918 to the present).

The editor avoided the strong temptation to concentrate on Nuremberg's great era when, as a powerful imperial city and the largest German territorial city-state, it produced skilled patrician statesmen; highly successful merchants and artisans; influential scientists, cartographers, and world travelers; widely known humanists, reformers, preachers, and teachers; and famous writers, artists, and musicians. Having given attention to the outstanding achievements of this golden age, the authors approach the subsequent history of the city, not as a decline in moral and creative strength, but as change brought about by powerful national and international forces. They show how the masses of the twentieth century replace the individual personalities of the sixteenth while personalities emerge from these masses only by virtue of intense specialization. The vitality of Nuremberg in the latter half of the twentieth century is evinced not only by the pictorial documents, which illustrate the emergence of the city almost totally destroyed during the Second World War, but also by the historical portrayal of its recovery from the problems created by the fateful Hitler years and two world wars.

The political history of Nuremberg is presented as a constant struggle with strong outside forces that threatened the existence of the city and its surrounding territory, from conflicts with the plundering Franconian knights and ambitious territorial princes and the destruction by friends and foes alike during the Thirty Years' War to the Napoleonic wars, which ended with its incorporation into the kingdom of Bavaria. Its subsequent vicissitudes are presented in their broader imperial, national, and international settings until it emerges as a vigorous city in a revived Germany.

The economic and social developments are traced from the acquisition of special market, mint, and customs rights, the emergence of a

vigorous patrician class, and the development of crafts and wealthy merchant companies to the growth of a lucrative trade with Italy, Hungary, the Low Countries, England, and northern Germany. There are excellent accounts of the city's efforts to maintain its prosperity in the face of disastrous wars, the development of mercantilism, the rapid industrialization of the nineteenth century, and the rationalization and competition of the present century.

Since Nuremberg was one of the first imperial cities to become Lutheran, the editor provides an excellent account of its role in the Reformation. But he does not neglect subsequent religious history for the authors trace the influence of pietism, rationalism, romanticism, and liberalism on the city's religious life and give attention to contemporary religious currents.

The volume devotes adequate space to the discussion of the cultural achievements of Nuremberg during its era of greatness but also calls attention to those institutions that today cherish these achievements and at the same time invite and experiment with the new. It describes the important efforts being made to further education and the appreciation of past and present literature, drama, art, and music.

Presenting the history of the city in a series of eighty essays written by thirty-eight experts results in some unevenness and repetition. Such minor defects are, however, offset by the fact that every facet of its development is covered authoritatively. The historian as well as every reader concerned with the highly complex phenomenon, Nuremberg, will read the volume with profit. It contains a complete bibliography and an excellent index.

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HEINZ STOOB. *Forschungen zum Städtewesen in Europa*. Volume 1, *Räume, Formen und Schichten der mitteleuropäischen Städte: Eine Aufsatzfolge*. Cologne: Böhlau Verlag. 1970. Pp. xi, 329. DM 58.

Nine articles, which originally appeared between 1956 and 1969, are reprinted here as an introduction to a proposed three-volume history of the towns of Central Europe through the eighteenth century. The essays concern two basic themes: town origins and the relationship of territorial princes to town foundation and

development. Stoob advances a chronology and generally corresponding typology of town development. Most centers originating before 1150 were organic "nuclei" towns that developed into great cities. The next century was dominated by larger planned urban foundations. "Small towns," generally planned, include most new towns appearing between 1251 and 1300, while the period from 1300 to 1450 was dominated by *Minderstädte*, tiny places referred to in the sources by terms indicating urbanism. The early modern period knew few town foundations but witnessed the emergence of several new urban types, such as garrison towns, foundations of religious refugees, and princely residences.

Stoob devotes four articles to the relations of territorial lords with their cities, particularly in Westphalia. He notes competition of rival town lords as a prime reason for the extreme complexity of urban structures in medieval Germany and argues that although large towns hindered centralization of the state, princes were able to counter this to some degree by the establishment of new towns.

Much material of interest is presented here. Most of the articles demonstrate the author's overriding interest in urban topography. But he focuses on essentially unoriginal descriptions of political events. He tends to take source terminology at face value, thereby broadening his concept of the town to the point of unworkability. His criteria of urbanism change between essays. He does little with urban institutions or legal development and gives absolutely minimal consideration to economic factors. Stoob's statistics of town foundation by time periods are useful, but his conclusion that the princes planned it that way is less than enlightening. His methodological expositions are strikingly simplistic and verbose. There is some repetition, and the author himself notes that much subsequent literature on these very questions must be considered in his second volume. A single comprehensive monograph might have obviated some of these problems.

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JEAN DELUMEAU, editor. *Histoire de la Bretagne*. ("Univers de la France": Collection d'histoire régionale.) [Toulouse:] Privat, Éditeur. 1969. Pp. 542. 66.80 fr.

MICHEL DE BOÜARD, editor. *Histoire de la Normandie*. ("Univers de la France": Collection d'histoire régionale.) [Toulouse:] Privat, Éditeur. 1970. Pp. 540. 72 fr.

PHILIPPE DOLLINGER, editor. *Histoire de l'Alsace*. ("Univers de la France": Collection d'histoire régionale.) [Toulouse:] Privat, Éditeur. 1970. Pp. 526.

Three recent additions to the Collection d'histoire régionale, under the general editorship of Philippe Wolff, maintain the high standards of historical scholarship and synthesis attained in the earlier volumes. Brittany, Normandy, and Alsace now take their place beside Languedoc (*AHR*, 74 [1968]: 624-25) with the promise of a complementary volume of documents for each provincial history. The Éditions Privat at Toulouse have produced splendid examples of craftsmanship. Each of these five hundred-page volumes is beautifully bound and well garnished (without being overladen) with maps, charts, tables, and an excellent choice of artistic reproductions and photographs ranging from Merovingian helmets and Rhenish miniatures to Breton châteaux and aerial views of St. Nazaire, frequently in brilliant color. Annotated bibliographies accompany each chapter.

The editors have brought together an outstanding group of local historians—most of them attached to provincial universities such as Rennes, Caen, and Strasbourg—to execute regional histories of unusual harmony. The danger of fragmentation into isolated historical essays has been largely avoided by assigning a substantial core of comprehensive chapters to three or four historians. For example, in the volume on Normandy, Michel de Boüard and Jean Favier have written the four chapters on the Middle Ages, Pierre Chaunu (and associates) the four chapters on the Old Regime, and Jean Vidalenc the three chapters on the nineteenth and twentieth centuries. For Alsace the corresponding historians are Philippe Dollinger, François Rapp, Georges Livet, and Fernand L'Huillier; for Brittany, Henri Touchard and Jean Meyer, to whom must be added the special contribution (three chapters) of André Mussat to the history of Breton art.

The scope and depth of these provincial histories is suggested by the amount of text allotted to different periods, no doubt by prearranged plan of the editors and authors. One-

half of the volumes on Brittany and Normandy concern the period before 1500 and about one-quarter of each volume treats the period before 1100. In the volume on Brittany, there are sixty pages on "pre-history," seventy on the French Revolution, and thirty on the post-1914 period, indicating what historical depth and longevity the provinces possess. About one-third of the volume on Alsace concerns the pre-1500 period and another third treats the period from 1500 to 1789. The greater emphasis on the "modern period" here is related to the impact of Renaissance humanism and the Lutheran Reformation on this Rhineland province, which was a part of Central Europe during eighteen of the twenty centuries of its recorded history. The post-Revolutionary period is also more detailed since Alsace became a major source of contention with both the Second and Third Reich.

Available documentation and research interest also determine the organization of each volume. The nineteenth and twentieth centuries receive less attention not only because the provinces have less identity after 1800, but also because research materials are better organized for the earlier periods in the local archives. Consequently, the richness of the earlier history has not been overwhelmed by modern electoral analysis and railroad statistics. Despite the commercial activities of the Alsatian towns or the Breton ports, one is still very much in a rural world. And here French scholarship shines its brightest. Demography, the study of social groups, economic growth, and collective psychology, along with the more traditional French emphases on human geography and a social history of ideas that includes popular literature and folklore are all marshalled for the task at hand. A general knowledge of political and military history is assumed, and narrative is subordinated to a description of the social and economic structure, institutional developments, and ideological movements, punctuated by interpretation and historical debate.

It is patently impossible in this review to set forth the "main themes" or "*grandes lignes*" of three provincial histories over two thousand years. Needless to say, all of the chapters are of very high quality. Special attention should be drawn to the treatment of "pre-history" in these three volumes. Here the archeologist replaces the historian of written documents, and

the reader sees how excavations, vestiges, temples and fortifications, cooking vessels and hand tools, place names and graveyards, and epigraphy and linguistics are all drawn upon to reconstruct a plausible historical evolution. Several chapters deserve special praise for their synthetic excellence. Philippe Dollinger's chapter on the late Middle Ages in Alsace ranges from the Black Death to the flagellants and the mystics, from town alliances to guild conflicts, from the river trade to wine production, and from ecclesiastical division to Dominican reformers—a triumph of historical integration. Pierre Chaunu and the "Caen group" re-create Norman village customs from notarial records, parish registers, and even from peasant furnishings and cottage design. Jean Meyer, equally at home on the Breton coast or in the hinterland, evokes the dynamic commercialism of "Malouin" and "Nantais," side by side with the most retrograde system of sharecropping, one world of magic and witchcraft, another of reforming bishops and parliamentary jurists, and another of popular Catholicism and an elite culture—a Brittany, not of isolation and mysticism, but of sharp contrasts between the novel and the traditional. Two other chapters should be cited, not for their scope and integration, but for their precision and objectivity: Roland Marx's chapter on the Revolutionary generation in Alsace and Fernand L'Huillier's chapter on Alsace in the Reichsland, 1871–1918.

These fresh provincial histories have avoided the "provincialism" of earlier works, dominated old quarrels and stereotypes, and proved that the history of France, until recent times, is the comparative history of very different regions. A thorough knowledge of the sources, a conscious use of new approaches, and a literary style characterized by compactness and integration are infused with verve and imagination grounded in a deep, almost instinctual knowledge of the locale. The inspiration of Marc Bloch and Lucien Febvre has not fallen on barren soil.

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LYNN GLASER. *Engraved America: Iconography of America through 1800*. Philadelphia: Ancient Orb Press; distrib. by Roy V. Boswell, Beverly Hills, Calif. 1970. Pp. viii, 76; 221 plates. \$110.00.

JOHN D. MORSE, editor. *Prints in and of America to 1850*. (Winterthur Conference Report 1970.) Charlottesville: University Press of Virginia for the Henry Francis du Pont Winterthur Museum. 1970. Pp. xx, 355. \$4.50.

Most American historians must plead guilty to a nearly total ignorance of the significance of prints as a source for the study of American history. The two books here reviewed help to remedy that situation. The first is a huge, luxurious compendium of the earliest engraved renditions of people, places, and things in America. The second is an excellent collection of analytical studies of *Prints in and of America to 1850* presented at the Sixteenth Annual Winterthur Conference, held March 19–21, 1970. It is necessary, first of all, to distinguish original drawings from prints. The main function of prints, as Jonathan L. Fairbanks, chairman of the conference, points out in his introduction, is communication. The importance of the process of producing exact multiple printed images, as another scholar, William M. Ivins, Jr. has pointed out in his book, *Prints and Visual Communication* (1969), is that it provided a necessary support for the revolution in science and technology with its requirement of exact description accurately and quickly communicated over long distances. "It is hardly too much to say," Ivins has asserted, "that since the invention of writing there has been no more important invention than that of the exactly repeatable pictorial statement." Yet the meaning of prints, let alone the technical processes involved in creating and disseminating them, has never been fully comprehended by scholars. The Winterthur Conference makes a significant step in meeting this need. Numerous specialists illuminate hitherto obscure aspects of the print medium. To single out only a few, Frank Sommer, in "Thomas Hollis and the Arts of Dissent," expertly delineates the relationship between Thomas Hollis, the English benefactor of Harvard College, and the creation of a symbol of British Liberty (an enthroned female with a pole supporting the *pilleus libertatis*), which entered the consciousness of Americans through widely disseminated prints. Peter Marzio, in his "American Lithographic Technology before the Civil War," notes that although Americans looked forward to lithography as a great democratic medium

for providing "pictures for everyone, . . . not one American contributed a new important idea for the design of presses or the use of stones before the Civil War." In the period after the Civil War "the artistic emphasis vanished" and lithography became a technical tool for business. Charles B. Wood's "Prints and Scientific Illustration in America" deals with the earliest use of prints for scientific illustration. Wood discusses the earliest American experiments in colored scientific illustration by aquatint, hand-colored intaglio engraving, and chromo lithograph. Richard Ahlborn deals with the beginnings of print making in Mexico in the sixteenth century while Richard B. Holman and Sinclair Hitchins discuss its Anglo-American beginnings in the seventeenth century. Elizabeth Harris's "Jacob Perkins, William Congreve, and Counterfeit Printing in 1820" is an extremely competent description of the methods by which banks sought to foil the ever-hopeful forger.

Engraved America, by contrast, is a lavish personal production by a young Philadelphia-area bibliophile and collector who has previously issued a book on *Counterfeiting in America* (1967). Mr. Glaser's aim is to illustrate "America as it looked or was believed to look" in the sixteenth through eighteenth centuries. He is not interested in portraits of European settlers, depictions of plants and animals, cartoons, or maps and city plans (all subjects of great interest to others). So he emphasizes Indians, landscapes, and city views from the whole hemisphere but with strong representation of Eastern North America. He presents only a sampling, not attempting to include all possible sources for any category of illustration nor even to exhaust the relevant contents of any one volume. Still he manages to reproduce 640 separate prints.

Perhaps the worst defect of the volume is Glaser's omission of what he calls "unnecessary material"—that is, exactly the sort of information necessary to make this collection a really useful index and guide for historians in search of data or illustrations. Nearly always at least a hint is given as to the source of the illustrations, but in most cases the precise identification of the volume and the page would require extensive search in specialized bibliographies and in a good library. Never does Glaser tell us

what repository he depends on; some of the illustrations may be from works in his own collection, but the originals of others are so rare that this is improbable. Glaser does not give the sizes of the originals, making it difficult to estimate the amount of detail lost through reproduction. He usually does not include information on filiation, states, and editions. It is obvious from his text that he has used a great many of the appropriate secondary sources—bibliographies, catalogs, and various other art-historical works—but he has not cited this material, which is widely scattered and difficult to learn to use.

Glaser reproduces seven cuts from Oviedo (pl. 7 and p. 17), but does not indicate which editions they are from. Those on plate 7 come from the 1547 edition of Oviedo's *Historia general*; there are earlier woodcut versions in the 1535 edition while the original drawings for three of them survive in the Huntington Library. However, the "oldest printed picture of an ear of maize from Oviedo" reproduced on page 17 comes from an Italian translation of Oviedo's *Sumario* and is not, in fact, the earliest depiction of maize. By not citing the precise source of his illustration, Glaser has missed an opportunity to help tie up some loose ends left in the literature on the earliest pictures of maize. On plate 8, items B and E are from Gómara, as Glaser says, but he does not tell us that they do not come from the first edition of 1552—a pity, for the version of E in that edition is the earliest printed illustration of the North American bison. Plates 11 A and B are from the first edition of the *Crónica del Perú* by Pedro de Cieza de León, published in 1553. Glaser garbles the author's name, omits the title of the book, and neglects to give the date of the first edition (although for once he tells us which edition he is reproducing here). Plate 12 includes several of the marvelous woodcuts, of great ethnographic value, that illustrate the history by Hans Staden (not "Staten") of his captivity among the Tupinamba; Glaser should have specified that A, D, E, F, and G are from the true first edition of Marburg, 1557, while B is not. On plates 17–20 it is not made clear which of Thevet's books is the source; D, and probably also E and F, are from his *Singularitez* of 1557 or 1558, rather than his *Cosmographie universelle*, while figure T was

modified by Thevet's engraver from an illustration in Staden. The text for plate 21 points out that Lery's figure F is related to an engraving in Thevet, but does not notice that figure A is related in several respects to the Thevet illustration reproduced as plate 20 U. This is only one of several examples indicating defects in Glaser's visual memory. The undated engravings on page 22 and plate 22 B are from the 1580 and 1578 editions respectively. De Laet's "Central American Indians" shown on plate 54 C and D are rather an Iroquois and a Montagnais copied from Champlain (whose versions Glaser reproduces on plate 59 and 62). On plate 72, figure C is not of Indians of New Netherlands but rather of Brazilian Indians, while the man in A is after the Susquehannock man on John Smith's map (pl. 75 here), which in turn is a modification of de Bry's engraving after John White (pl. 26 D here). Plates 109–11 are said (p. 41) rather vaguely to be copied from earlier works; 110 E and H go back to du Tertre (compare pl. 124 and pp. 46, 47) while 111 I is from A. F. Frezier, *Relation du voyage* (1716). The earlier, unspecified source of the Patagonian giants in plate 113 F is the frontispiece for the 1767 French edition of John Byron's *Voyage round the world*, which is derived in turn from a less romantic version in the first and second 1767 English editions of the same work (both printing from the same plate). Glaser does not know the source of his plate 138 A—it is Bernard Picart, *Ceremonies et coutumes religieuses* (Amsterdam, 1723). Plate 143 reproduces the superb mezzotint portraits of the Four Kings of Canada who visited Queen Anne in 1710. Glaser should tell us that the example at lower left is of the second state, while the other three are in the third state. It would be nice to have reproductions of the first state, and it might have been mentioned that the original oil portraits (or closely related ones) survive in England. The illustrations in the original account of James Cook's third voyage are beautiful examples of the engraver's art, but the reproductions on plates 217–19 here are rather debased derivatives from some later edition, despite the implications to the contrary in Glaser's text. Here, too, paintings and drawings underlying the engravings survive. Plate 216 D is called an "Indian fort," although in the book from which it comes Ber-

nard Romans clearly described it as a Choctaw burial platform. It would be easy to lengthen this list of important omissions and errors.

Glaser's is a very attractive volume, with huge pages nicely printed on good paper in a pleasing layout with generous margins (useful for the annotations and corrections any serious user will want to add). Neither author nor printer nor any proofreaders know how to spell. Good representatives of prints were usually chosen for copying (one exception is the badly torn example reproduced as plate 157-58), and the quality of the reproductions is generally excellent—much better, for example, than those issued by the Akademische Druck- und Verlagsanstalt, Graz, in its recent series of expensive reproductions from illustrated books of this period.

To some extent these two works complement each other. Scattered in the Winterthur volume can be found citations of some of the standard reference works Glaser omits (although Morse could have assisted us more by including a guide to this literature to accompany Wendy Shadwell's useful annotated list of print repositories). Glaser recognizes that it was the Indian above all that made prints of America different, and he includes a fine array. The Winterthur volume by omitting this subject makes prints "in and of America" seem mere peripheral colonial reflections of European printmaking. Morse includes discussions and examples of botanical and zoological prints, a topic unjustly skimmed by Glaser. Such illustrations may still be of scientific importance, for the descriptions and specimens on which Linnaean names rest often include engravings. Further, precisely because the appearance of the living models is known and unchanging, such prints are useful sources on changing artistic styles and on the nature of the reaction to exotic subject matter. For similar reasons the treatment of the anatomy and postures of foreigners must be evaluated separately from the artifacts, including clothing and ornaments, shown in the same illustrations.

Despite its flaws, the Glaser volume ought to be purchased as a reference tool by many libraries. The Morse paperback advances the historical study of prints more directly and immediately.

With these and other tools, we may soon ex-

pect works on the iconography of America as fine as, for example, Bernard Smith's *European Vision and the South Pacific, 1768-1850, A Study in the History of Art and Ideas* (1960), P. H. Pott's *Naar wijder horizon: Kaleidoscoop op ons beeld van de buitenwereld* (1962), or Donald F. Lach's *Asia in the Making of Europe*, volume 2: *A Century of Wonder*, book 1: *The Visual Arts* (1970).

WILLIAM C. STURTEVANT
WILCOMB E. WASHBURN
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DONALD F. LACH. *Asia in the Making of Europe*. Volume 2, *A Century of Wonder*. Book 1, *The Visual Arts*. Chicago: University of Chicago Press. 1970. Pp. xvii, 257, 151 plates. \$12.50.

This is the first part of the second volume of Professor Lach's comprehensive study of Asian impact upon Europe. Its scope is wider than the title suggests. Though centered upon the sixteenth century, pre-existing situations, where relevant (for example, Italian contact with the East), are first surveyed; and the object of the enquiry is wider than "visual arts" suggests. It extends in fact to all the "silent sources," that is to say, art objects, artifacts, flora, fauna, and crafts brought into Europe by traders, travelers, missionaries, and others. "I asked myself," the author writes, "What were they? Who received them? What questions did they provoke? How did artisans and artists in Europe see them?" He then turned for the answers to cargo lists, travel books, inventories of collections, and emblem books, as well as the more usual sources.

The result is a wholly admirable study: the most thoroughgoing and studiously empirical conspectus yet undertaken of the transmission of Asian artifacts, goods, and fauna to Europe and their effects upon ideas, attitudes, and design. The movement of Asian exotica is traced from merchandising centers like the Rua Nova dos Mercadores, Lisbon, to the Antwerp Portuguese market (which flourished between 1503 and 1553), and thence to the markets of Italy and Central Europe. Florence (welcomed by the Portuguese) replaced Venice (excluded by Portugal from its spice trade) as the leading distributor of Asian goods in Italy. In Central Europe the enthusiasm of the Habsburgs and the great merchant princes, the Fuggers, Wel-

sers, and Prauns, made the collections of Germany richer and more comprehensive than those of other countries. This was the century of the *Kunst* and *Wunderkammer*. Rudolph II, with agents everywhere, made Prague the *Wunderkammer* of Europe, dividing his great collection systematically into *naturalia*, *artificialia*, and *scientifica* in order to establish contrasts and comparisons between the handiwork of nature and that of man.

At the level of curiosity and science Lach has little difficulty in demonstrating how the products of Asia led gradually to an increasing acceptance of the reality of Asia and to the need for a more universal and systematic, less Eurocentric view of man and the universe.

But in his consideration of the reception of the Asian arts by European artists one notes a difference between what the author appears to be seeking and what, cautious historian, he finds. "The European artist, then and now, was more likely to be curious about the works produced by his opposite numbers in Asia than by exotic flora, fauna and miscellaneous oddities," he writes. Yet Dürer's main excursions into Asian exotica—and he was drawn more to the East than any of his artist contemporaries—took the form of drawings of Asian parrots and monkeys and of the famous woodcut of the rhinoceros sent to Manuel I of Portugal. Raphael drew, as seems most probable, Manuel I's elephant Hanno for Pope Leo X. Where major artists do enter into Lach's account, artists such as Altdorfer, Arcimboldi, Cock, Savery, and Annibale Carracci, it is usually because of their depiction of Asian animals, either directly as illustrations of exotic natural history or in appropriate, traditional contexts such as the Four Continents, the Triumph of Bacchus, the Garden of Eden, the Adoration of the Magi and its Eastern kings, or Orpheus charming the Animals. It was, it would clearly appear, even on the basis of Lach's own account, the fauna and the oddities of Asia rather than the art that aroused the sixteenth-century European artist's keenest curiosity.

Here the evidence of architecture is relevant. Lach, after considering the one major claim for an Asian influence upon sixteenth-century European architecture, the case of the Portuguese Manueline style, finds it lacking in substance.

Costa Torres's case for an Asian origin of the baroque is likewise rejected. "Asia was still too remote to be seriously considered in any search for architectural models." But when one recalls the great sweep of the animal style across the Eurasian steppe in prehistoric times and the penetration of Byzantine art through early medieval Europe, one doubts that remoteness is the answer. Not remoteness, surely, but choice: the proud preference for naturalism based upon the authority of Mediterranean antiquity. "What about the great masters of the late Renaissance?" asks the author at the beginning of his book. "Were they so imbued with the artistic prototypes of Antiquity and their own spectacular achievements that they were totally oblivious to the opening of the overseas world?" Well, based on his own findings they were not totally oblivious but almost so. Asian art qua art did not deflect the major styles or the major personalities in their development. Michelangelo accommodated Asia within his concept of antiquity. Raphael and his assistants, Giulio Romano and Giovanni da Udine, incorporated Asian motifs into the grotesque style born of their passion for the Roman decorative art of the Domus Aurea. The sixteenth-century artists accommodated all things Asian within a more comprehensive but still thoroughly European view of the world. The Christian and humanist hubris remained unshaken.

For Europo-Asian cultural contact in the sixteenth century the key to the problem surely lies in the fact that Asian exotica were portrayed by Europeans naturalistically. They imitated the objects not the styles. Asian exotica augmented the iconographic and symbolic repertoire of the European decorative artist but did not, at this stage, seriously divert him from his naturalistic predispositions. Where style, design, and taste did affect the European arts it came via the emulation of technical achievements in which Asia was superior to Europe: in porcelain, textiles, and lacquer work. Here the desire to emulate and master the techniques led naturally to the copying of designs and patterns. The Medici "porcelains" of Francisco I, the fashion for ebony furniture, and the experiments with lacquer ware find skilled European craftsmen at work, emulating for the first time after many centuries superior Asian models. Where the applied arts led, the

fine arts later followed. An Anglo-Asian hybrid style in the eighteenth century—inadequately called *chinoiserie*—was one of the major factors that led to the triumph of the decorative over the naturalistic.

Perhaps it is to this that Professor Lach is referring when at the conclusion of his admirable book he writes: "The clear stream of Western, Christian civilization still flowed strongly, but occasional streaks in its waters gave evidence that erosion of its banks was beginning."

BERNARD SMITH
University of Sydney

J. J. EDDY. *Britain and the Australian Colonies, 1818-1831: The Technique of Government*. New York: Oxford University Press. 1969. Pp. xvii, 326. \$8.75.

J. J. Eddy has had the excellent idea of writing a book on Australia that is also a case study of the working of British government in the period between Waterloo and the Reform Bill. No section of the British community was subject to such continuous supervision by so many departments of the central government in this period of "leisurely experimentation" when Tory governments responded unsystematically but with "openness and fairness" to the pressures of Parliament, public opinion, the colonists, and the members of the emerging civil service. From being a largely state-managed economic, monetary, and social community in 1818 the Australian colonies became recognizable as colonies of settlement, like those in North America, with a free press, trial by jury, and colonial government control over locally raised revenues. They also moved cautiously in the direction of a free market economy, a thrifty and efficient administration, and an ecclesiastically plural society.

Nonspecialists will find this book difficult, with a topical approach that makes few concessions to readers not already thoroughly familiar with the main story line. It is gentle and kindly, enlivened by a selection of apt anecdotal quotations, and it brings together a great deal of information from a wide variety of sources. Apart from an occasional failure to make the distinction between the personal opinions of officials and the opinions they expressed in official documents, the scholarship is careful and reliable.

Robert W. Hay, the permanent undersecre-

tary at the Colonial Office, cast in a recent history of British bureaucracy as the archetype of the incompetent senior civil servant produced by the jobbery and nepotism of the unreformed civil service, emerges as a creditable administrator and a friend of Australian freedom. Eddy, like Hay, accepts the Tory viewpoint that problems "required for their solution expensive paternalistic plans to aid the common people which *laissez faire* ruled out." On church-state relations he is cautiously ambivalent, asserting that "Bathurst and Bigge cannot altogether be deemed the conscious 'architects' of a conservative grand design" to remodel the social framework of the colonies.

The stark contrast between the government's policy of amity and kindness and the slaughter of "the fierce, gentle, mysterious, fugitive, ever-present people of the Australian 'bush'" serves as a reminder of the gap between the expression of ideas and their implementation. The chapters on Parliament and the press reflect the author's mastery of recent scholarship, and one comes away with a clear idea of opinions and political ties but not of exactly how, or how much, Australian lives were affected. Administrative historians will welcome the sections on Treasury control and on the working of the Commissariat. The sections on land settlement and sub-imperialism say little that is new. Altogether it is a useful book to place on the shelf beside D. J. Murray's study of the West Indies. Students planning studies of other colonies should use Murray as a model.

D. MURRAY YOUNG
University of New Brunswick

DOROTHY R. ADLER. *British Investment in American Railways, 1834-1898*. Edited by MURIEL E. HIDEY. Charlottesville: University Press of Virginia for the Eleutherian Mills-Hagley Foundation. 1970. Pp. xiv, 253. \$11.50.

Fortunately there are several sorts of economic history nowadays, each viable in its own terms. Dr. Adler's study mainly, but not exclusively, answers the question: How? How were Anglo-American capital markets organized? How did traders, brokers, and bankers operate? She is concrete and detailed; we learn how things were done and who did them. This is the fruit of deep, painstaking research among difficult materials.

Dr. Adler distinguishes three periods (the

1840s-50s; the 1860s-70s; and the 1880s-90s) through which she traces the ups and downs of the British market for American securities and the growing sophistication of the mechanisms concerned. External financing of U.S. railroads was from the outset bound up with the relative backwardness of the American iron industry and exports of British iron (iron and bonds were even exchanged). A large transfer of British technology and funds took place to create the American railroads. Initially British investors chose rails for growth rather than income investment; *rentier* investors, after all, could depend on a steady six per cent from U.S. government securities, so why risk railroads? The bulk of British investments, four-fifths, remained in government stock as late as 1860. Railroad investors preferred shares to bonds throughout the 1850s, until the advent of the convertible bond (and the even more popular land-grant bond) promised the best of both worlds: equity and income. Erratic secular growth in the 1860s and 1870s saw extension of British interests to related native American industries now developing—coal and iron. Despite setbacks like the Erie War, British interest in American rails surprisingly persisted. More adventurous investors (like earlier counterparts in Illinois of the 1850s) now began to develop the South and the Southwest, a story of British influence that is itself worth a future book.

Merchant banks dominated the market by the 1870s, seven large houses in particular. New, risk-spreading investment trust companies proved very successful. As U.S. rails hit the height of popularity in London in the 1880s, techniques emerged to safeguard investors. These ranged from outright British acquisition of U.S. lines, to representation on boards of directors, or the more effective creation of "protective committees" in London exercising varying degrees of operational control in such matters as rate wars and mileage.

Dr. Adler's book goes off in various directions; all her asides are informative, and her sensitive editor, Muriel Hidy, was wise to retain them. The book is elegantly produced and worthy of the memory of the author, whose life was cut short in 1963.

PETER D'A. JONES
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JOHN E. SCHRECKER. *Imperialism and Chinese Nationalism: Germany in Shantung*. (Harvard East Asian Series 58.) Cambridge, Mass.: Harvard University Press. 1971. Pp. xiv, 322. \$12.00.

Here is a well-presented case study of German imperialism in Shantung, one of the important coastal provinces in China. It reveals in chronological order a complete story starting with Germany's acquisition of a leasehold at Chiao-chou Bay, continuing with the actual development of the leasehold, the gradual extension of German influence throughout the province, and China's response to the German threat, and ending with the disintegration of the German sphere of influence. Following this account, there is an objective evaluation of the German accomplishments at Tsingtao as well as in the entire province.

There have been indeed numerous writings on the Shantung problem as a provocative issue during and after the First World War, but there has been little information about the actual operation of German imperialism in Shantung before 1914. Mr. Schrecker's book definitely bridges a wide gap in the study of Western imperialism and China's response in the modern period. Furthermore, it is based on thoroughly digested Chinese and German source materials practically unused so far. It is mainly for this reason that it is a significant scholarly contribution.

Mr. Schrecker's work represents, among other things, an attempt at reinterpreting China's foreign policy of the late Ch'ing period in terms of her relative success in handling the problem of Western imperialism. Too many scholars, both Chinese and Western, have had the tendency to overemphasize the decadence of Chinese traditional values and the corruption and incompetence of the Ch'ing government. The fact that China stood undismembered in the face of the powers' scramble for concessions and spheres of influence has usually been explained in terms of such factors as the American Open Door Policy, self-defeating jealousy among the powers themselves, their preoccupation with more serious problems elsewhere, and what not. The resilience of Chinese culture, the ability of Chinese officials, and, what is most important, the rapid spread of nationalism among the Chinese people, particularly the intellectuals, have been too often belittled or ignored as factors contributing to

the restraint of foreign influence in China. Failing to understand these factors, one would find it extremely difficult to explain late Ch'ing China's comparative success in recovering and defending her sovereignty, preventing further local attacks upon foreign missionaries and merchants, refusing to grant more economic concessions, building up modern military power for defense against aggression, and tactfully utilizing international rivalries to her advantage. Mr. Schrecker has especially stressed these factors, and his approach is undoubtedly sound.

Up to 1900 China's two alternative approaches to foreign affairs, the so-called mainstream and militant conservative approaches, had played an important role but had failed. The third alternative, the nationalist approach, which had been staunchly advocated during the One Hundred Days' Reform of 1898, finally replaced the first two and became the undisputed policy of the day. Strangely enough, from the end of the Boxer Rebellion to the fall of the Ch'ing dynasty it was not the handful of reformers of 1898 but mainly some of the influential supporters of the mainstream and conservative approaches both in the central government and in the provinces that were trying to carry out the reform measures previously advocated by the reformers. All these people, confronted with a critical situation in China, could not help but be driven to this new nationalist position. The spread of nationalism in China and the numerous people involved in the implementation of the nationalist policy present a far more complicated picture than usually envisioned or described. It is a picture that warrants further and more penetrating investigation, and Mr. Schrecker has laid a good foundation in this regard.

TIEN-YI LI

Ohio State University

Z. A. B. ZEMAN. *The Gentlemen Negotiators: A Diplomatic History of the First World War*. New York: Macmillan Company. 1971. Pp. xi, 402. \$9.95.

This general survey of First World War diplomacy displays considerable flair, though it shows signs of having been rather hastily written. Professor Zeman develops no brilliant and provocative themes, but he describes many

events and issues cleverly. The tales of folly and futility are enlivened with brief personality sketches, and various episodes are highlighted with interesting detail. The table of contents reveals an intriguing scheme for organizing the narrative. Each chapter bears the name of a wartime capital, with the exception of one on Brest Litovsk. Professor Zeman makes this difficult plan work for the most part, though he runs into intractable problems. The chapter entitled "Paris," for instance, bears no special relationship to France.

Any general history necessarily omits points of interest, and this work is no exception. Professor Zeman's coverage of the question of peace negotiations with Austria-Hungary provides several examples that are somewhat surprising in view of his professional interest in that empire. He does not show how Britain's Hopwood mission in Scandinavia, which was investigating Austrian peace feelers, was related to President Wilson's simultaneous attempt to mediate between Britain and Austria-Hungary. Lloyd George held Wilson off until he felt sure that the Hopwood mission would be fruitless, and then let the president have a try. Professor Zeman also states (p. 159) that the close of the year 1917 put an end to Austrian peace feelers, except for a futile meeting between Count Abel Armand and Count Nicholas Revertera in February 1918. This is not accurate. For instance, in January 1918 the Austro-Hungarian foreign minister suggested an exchange of views with America as a starting point for wider peace negotiations. In February Emperor Karl sent a message to President Wilson through the king of Spain in which he proposed direct discussions between his own representative and one of the president's. These omissions contribute to Professor Zeman's neglect of another interesting aspect of the question, namely, the remarkably long time that hopes of a separate peace with Austria persisted. There is no reference to Philip Kerr's mission to Switzerland in March 1918, which followed up some Austrian feelers. Historians will also notice the usual number of minor slips; Lord Curzon, for instance, is confused with Walter Long (p. 326). They will probably question various omissions from the select bibliography as well.

Such faults do not ruin a general history,

and Professor Zeman's work remains both enjoyable and informative. There is, however, one observation that should be made in reference to the publisher's claim that his book satisfies the need for a general diplomatic history of the First World War. Professor Zeman apparently finished this work in 1969. Since then, a large amount of research has been completed, especially on the British documents that have been opened to scholars in recent years. It would be very useful to have a general history that utilized the new books and theses that are now available.

STERLING J. KERNEK
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TREVOR R. REESE. *Australia, New Zealand, and the United States: A Survey of International Relations, 1941-1968*. (Issued under the auspices of the Royal Institute of International Affairs.) New York: Oxford University Press. 1969. Pp. 376. \$8.75.

A thoughtful history of recent American-Australian-New Zealand relations has been needed for some time. Dr. Trevor Reese, an accomplished writer on British Empire-Commonwealth subjects, has satisfied this need with an excellent survey, mainly from the perspectives of Canberra and Wellington.

The survey is unified by three themes: the replacement of Britain by the United States as the primary source of Australia's and New Zealand's security; the difficulties the two Commonwealth nations have had with American policies; and the tenacity with which they have maintained, despite irritations, their ANZUS tie with America.

Reese treats the first theme with scholarly balance. He discusses fears of American dominance and of the erosion of an essentially British way of life in Australia and New Zealand, but he also explains fully and fairly the emerging American orientation in foreign and defense policy. His chapter on "The ANZUS Alliance (1952)" is a model of diplomatic analysis based on public documents. It reminds us that the original Australian and New Zealand motive for ANZUS was less anticommunism than fear of a resurgent Japan, a point obscured by American cold war history.

The second theme should interest American readers, especially any who—like their govern-

ment—have taken Australia and New Zealand for granted. Actually, those two nations have disliked several important American policies: the conciliatory occupation of Japan, the implacable rejection of Peking, the exclusion of Britain from ANZUS, inconstancy during Suez, brinkmanship over Quemoy and Matsu, and opposition to Commonwealth tariff preferences. Australia and New Zealand have also received cavalier treatment by America during wartime, most recently in Vietnam, the subject of an interesting chapter. During that war, the United States has valued the ANZUS commitment mainly for its "international" appearance and has not consulted Canberra or Wellington about important decisions.

Why then have Australia and New Zealand been loyal to ANZUS? The orthodox reply is that Britain's gradual withdrawal from east of Suez left no alternative. Reese recognizes the merit of this argument and its appeal in countries with traditions of loyalty to a great power. He does not, however, leave the ruling dogma unquestioned. Deference to ANZUS, he says, "stultified imagination in Australian and New Zealand policy and led to a rigid, western-oriented attitude" (p. 146) and to neglect of opportunities for better relations with Asian neighbors. Nor is he uncritical of those who made the dogma. Robert Gordon Menzies, for all his brilliance and pluck, appears as somewhat melodramatic ("given on occasion to simulated Churchillianism" [p. 82]) and nearly outdated.

However, Reese's viewpoint never twists his careful narrative. Students of American diplomacy and of the Commonwealth should welcome such a necessary book, so well done.

ROBERT E. BUNSELMAYER
Fordham University

LEON BORDEN BLAIR. *Western Window in the Arab World*. Foreword by MOHAMMED EL FASI. Austin: University of Texas Press. 1970. Pp. xiv, 328. \$8.50.

I hope that the poetic title, *Western Window in the Arab World*, will not cause students of U.S. foreign policy and of contemporary Morocco to overlook this important and original study of the U.S. military presence in Morocco from 1942 to 1963.

Between 1954 and 1960 as a lieutenant-com-

mander in the U.S. Navy, Leon Borden Blair served successively in Morocco as an aviation technical adviser to the French Navy, U.S. Navy politico-military liaison officer, and technical adviser to the Royal Moroccan Armed Forces. From his experiences he has been able to develop some valuable material on the urban liberation movement of 1953-56, which is often minimized or misrepresented in accounts based on French sources. He also established useful contacts among Moroccan, French, and American witnesses to the events of the two decades. Several dozen documented interviews with them supplement his own recollections, the published literature in French and English, and materials from U.S. military archives and records, especially on controversial matters.

From Blair's investigations emerges a very colorful, readable account of the American landings and occupation during the war years and of the U.S. naval and air bases during the last decade of the French protectorate and the first six years after independence. Blair gives attention not only to the official military activities and to the diplomacy that affected them but also to what he calls "the social impact" of the Americans upon the Moroccans. He contends that the U.S. presence, which involved a variety of good works and benefits for Moroccans, produced very favorable attitudes among them toward the Americans and their country. At the same time official U.S. deference to France and its policies in Morocco hindered the development of good U.S. relations with Morocco both before and after independence. In the author's opinion the U.S. diplomats at Rabat failed to establish the kind of contacts with Moroccans that would have allowed them properly to report the local situation to Washington or to improve relations without injuring relations with France. He attributes the ultimate evacuation of all the U.S. bases except the naval base at Kenitra (Port Lyautey) more to the failures of U.S. diplomacy than to popular pressures within Morocco.

Blair expresses his opinions about the leading actors in his story in terms that are often implicitly moral. The U.S. Navy, the royal family, and Moroccans friendly to the United States win his praises; and the U.S. Air Force and diplomats, French Residency officials, and

the French military his criticisms or condemnations. His sharply expressed and carefully documented conclusions on U.S. policy in Morocco and Morocco's recent political evolution will probably generate further discussions both here and overseas.

DAVID E. GARDINIER
Marquette University

I. I. ORLIK. *Imperialisticheskie derzhavy i Vostochnaia Evropa (1945-1965)* [The Imperialist Powers and Eastern Europe (1945-1965)]. (Akademiia Nauk SSSR, Institut Mirovoi Ekonomiki i Mezhdunarodnykh Otnoshenii.) Moscow: Izdatel'stvo "Nauka." 1968. Pp. 286.

A. N. KRASIL'NIKOV. *Vneshniaia politika Anglii i leiboristskaia partiia (1951-1964)* [British Foreign Policy and the Labor Party (1951-1964)]. (Akademiia Nauk SSSR, Institut Istorii.) Moscow: Izdatel'stvo "Nauka." 1968. Pp. 434.

According to *Imperialist Powers and Eastern Europe*, the East European economic interests of the imperialist powers, including the United States, were the chief factors influencing the policies they adopted toward that area. Thus it is emphasized that banks of the United States and the imperialist powers participated in the recovery loan granted to Hungary in the mid-twenties through the intermediary of the League of Nations. Large-scale American holdings in the Hungarian bauxite industry are also emphasized along with the fact that the "largest factories in Silesia belonged to the American *billionaire* Harriman" (my italics). It is further charged that the Western powers made concessions to Germany in Eastern Europe with the goal of directing Germany's military might against the Soviet Union. A remark of Churchill's that he would "not have objected to a gradual growth of German influence in the Danubian basin" is interpreted to mean that Churchill had wanted to direct Nazi aggression against the Soviet Union. It is stated (p. 12) that in September 1939 Poland was seized by fascist forces; interestingly enough, no mention is made of the Soviet role in the events of that month.

Somewhat new is the assertion that in 1945 the United States subjected many industrial areas in Romania and Czechoslovakia to bombardment when it became certain that they would be seized by the Soviets within sev-

eral weeks. The implication is obvious that the United States even in 1945 did not wish to have the Soviets occupy functioning industrial areas.

One aspect of this book that will most likely trouble the historian is the lack of a true or orderly chronology. There is a continual skipping around over a period of several years, something that could possibly be deliberate. For example, it is mentioned (p. 81) that "Truman announced before the bombardment of Hiroshima that the bomb [A-bomb] is the decisive factor for deciding not only the Polish but also the Romanian, Yugoslavian, and Manchurian questions." Unfortunately no documentation is given for that statement and the author does not mention how Manchuria became a disputed factor before the Russian invasion, which came after Hiroshima.

For the most part the reader familiar with Soviet historiography concerning the postwar period will find little that is new in this work.

Much the same could be said about Krasil'nikov's *British Foreign Policy and the Labor Party (1951-1964)*. The author finds that the great successes of world socialism during the years of Laborite opposition, 1951-64, changed class relations in Great Britain and opened the door for a greater cooperation between the Labor party and the Communist party. However, the leaders of the Labor party and the trade unions rejected the numerous proposals of the Communists for joint action, and while there are "of course among the left elements in the Labor party and trade union not a few persons who are honestly struggling for peace and against the imperialist course in the foreign affairs of their country," their "fundamental weakness is that they act in isolation."

In short both books represent the official Soviet view on the twenty-year, postwar period; neither appears to contain, however, new documentation of any importance.

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RICHARD E. NEUSTADT. *Alliance Politics*. New York: Columbia University Press. 1970. Pp. xii, 167. \$5.95.

These Radner Lectures delivered at Columbia explore the diplomatic implications of bureaucratic politics in the framework of the An-

glo-American alliance at two critical moments, Suez in 1956 and the Skybolt affair of 1962. Basic to Professor Neustadt's analysis is the idea that government decisions are bargaining outcomes in a deadly serious power game among men whose offices mold their perspectives and define the stakes. This is "Conceptual Model III" of Graham Allison, to whom, with other colleagues at Harvard's Kennedy Institute of Politics, Neustadt pays his compliments. Internationally, he finds, bureaucratic politics invites misunderstanding. It is a self-contained process with rules, rewards, and penalties locally defined. Each capital, absorbed in its own game, slips into thinking the game abroad the same. In the peculiar Anglo-American relationship, where familiarity breeds complacent thinking, officials fail to note subtle differences in political accountability, procedure, perspective, and job-to-job relationships. Misperception leads to disappointed expectations, bitterness, and paranoid reaction. The result is spiralling crisis.

A few examples must suffice. In planning Suez Eden hoped for American passivity. Hope nourished false notions of constraints on Eisenhower, like the New York Jewish vote and the usefulness of Suez as a precedent for a like contingency in Panama. Meanwhile Washington, unversed in British politics, relaxed in the thought of Eden hobbled by his Opposition when in fact he was spurred on by Tory jingoes. In Skybolt Kennedy and McNamara underestimated Macmillan's political stake in the missile and his difficulty in altering a cabinet decision, so after warning of cancellation they simply waited for the "clever chaps" in London to ask for a substitute. Kennedy chose to kill Skybolt by elimination from his budget, while London, ignorant of American budgetary politics and deadlines, sat tight and learned of cancellation in the papers.

This is an intriguing study, taut and vivid in style and argument. An essay in method, it is meant to be suggestive not definitive. Nonetheless, Neustadt makes a substantial contribution to Skybolt, which he researched for President Kennedy, though he is not at liberty to reveal his sources. Among the fresh insights on Suez is a different Dulles, wary, dependent on Eisenhower, and meticulous in consulting him. Bureaucratic politics is so broad in compass that

Neustadt seems quite conventional at times. Nevertheless, it is a unique and rewarding way of looking at events, and historians cannot afford to ignore it.

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ANCIENT

I. E. S. EDWARDS *et al.*, editors. *The Cambridge Ancient History*. Volume 1, Part 1, *Prolegomena and Prehistory*. 3d ed.; New York: Cambridge University Press. 1970. Pp. xxii, 758. \$19.50.

Although entitled *The Cambridge Ancient History* this volume bears little resemblance to the preceding edition published almost a half century ago. In 1923 eight distinguished British scholars presented what was then known of human history in the two centers of ancient civilization through the times of the Hyksos and the Kassites in volume 1 of *CAH, Egypt and Mesopotamia to 1580 B.C.* Now the horizon of prehistory has become so enlarged that an even larger volume has been required to bring the record down to the advent of writing at the end of the fourth millennium. Eighteen specialists in particular problems, areas, and chronological periods have detailed what is currently known of man's prehistory and documented it with fifty pages of references that provide the serious student with the most comprehensive bibliography now available. Scholars from America, Canada, and France, as well as England, have written for the present edition. When judged by the competence of the contributors, the wealth of detail compressed into the text, the maps, charts, and bibliographies, this is by far the most useful presentation of man's history in the broad belt that extends from Greece to Iran from his earliest appearance to the end of the Chalcolithic period.

D. L. Linton and F. Moseley set the stage on the 4,500 million year-old earth for the appearance of the genus *Homo* less than two million years ago in a chapter on "The Geological Ages," and K. W. Butzer sketches the ecological conditions that existed before the period of human settlements some ten millennia ago. Following these prolegomena to prehistory D. A. E. Garrod describes the Paleolithic period

in Egypt, Syria, Lebanon, Palestine (where she distinguished herself by her work at Mount Carmel), and elsewhere, and J. G. D. Clark follows with a consideration of Mesolithic times in Europe, North Africa, and Southwest Asia. Here the story of man's development is interrupted by three chapters of prolegomena. W. F. Albright and T. O. Lambdin provide a survey of the languages spoken in the ancient world—it is estimated that spoken languages were already well developed at least one hundred thousand years ago. The evidence from physical anthropology for "The Earliest Populations of Man in Europe, Western Asia and Northern Africa" is given by D. R. Hughes and D. R. Brothwell in a summary fashion (only sixteen items appear in the bibliography). The treatment of chronology is fully and carefully documented in sections written by W. C. Hayes ("Egypt to the end of the Twentieth Dynasty"), M. B. Rowton ("Ancient Western Asia"), and F. H. Stubbings ("Aegean Bronze Age"). Unfortunately the reader must wait for the appearance of the next volume in the series for the synchronistic table that is to summarize the results of this painstaking investigation on the part of these three most competent investigators. Here and elsewhere in the volume carbon-14 dates are relied on with varying degrees of caution, sometimes without a notation of which figure for the half life of carbon-14 was used. Modifications in each of these dates will have to be made to accommodate the newly established correction factors based on a comparative study of precisely dated tree-ring samples and carbon-14 dates obtained from them.

J. Mellaart presents what the burgeoning archaeological evidence has revealed of the earliest settlements in Western Asia from the ninth to the end of the fifth millennium B.C. Up-to-date bibliographies and a table of prehistoric cultures make this section an extremely useful introduction to the subject, even though the necessary limitation on illustrative material, so essential for an understanding of archaeological evidence, limits the effectiveness of the presentation. The detail with which Mellaart presents "Anatolia Before 4000 B.C.," an area in which he himself has made significant discoveries, is apparent from the map of 109 prehistoric sites. The longest chapter in the volume is M.

Mallowan's "The Development of Cities from al-Ubaid to the End of Uruk 5." This is a masterful treatment by one who has contributed significantly to a knowledge of the period. Not only is this chapter a catalog of archeological data but it is a superb synthesis of what is known of the development of urban life in Babylonia, Assyria, Syria, and Iran. E. J. Baumgartel treats succinctly predynastic Egypt, and R. de Vaux has a compressed archeological history of "Palestine During the Neolithic and Chalcolithic Periods." De Vaux introduces a terminology of his own to replace the more conventional terms. Pre-pottery Neolithic is dealt with under the rubric "The First Settlements: Hunters and Farmers"; Pottery Neolithic, under "Farmers and Potters"; and Chalcolithic under "Farmers, Potters and Metalworkers." H. W. Catling covers the corresponding periods in Cyprus, although there the Chalcolithic period persists to 2300 B.C., some eight hundred years longer than it did in Palestine. The final chapter by S. S. Weinberg is on "The Stone Age in the Aegean," a treatment replete with three maps of sites and a bibliography that runs to nine closely printed pages.

Each of the essays in this volume was issued by the publisher over the past six years in a separate fascicle as a "revised edition" of *CAH*, and detailed critical reviews have appeared in the more specialized journals. Many points have been and will continue to be challenged and debated. Yet every serious student can be grateful for this presentation of what was known in the early 1960s from archeology and anthropology about the prehistoric periods over the wide area in which human culture developed. The average reader may wish for more synthesis and interpretation rather than for what may seem to him to be the listing of sites, periods, and artifacts. Yet the day in which Eduard Meyer could produce his monumental *Geschichte des Altertums* has long since passed. No one scholar can now encompass the mass of data that Meyer correlated for such a large area and that Erman, Breasted, Olmstead, Meissner, and Goetze organized for more restricted regions. The book, therefore, is limited in its use. It is more suited to the specialist than was volume 1 of the first edition. Yet it is a monumental attempt to compress a vast amount of factual data, to air honestly doubts

and uncertainties, and to state those conclusions about connections between peoples and cultures and developments in techniques and styles that seem plausible in the light of the available evidence.

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T. B. MITFORD. *The Inscriptions of Kourion*. (Memoirs of the American Philosophical Society, Volume 83.) Philadelphia: the Society. 1971. Pp. xvi, 422. \$20.00.

This is a corpus of inscriptions from a city on the south coast of Cyprus, illustrating its history from the seventh century B.C. to the late fourth century A.D. Some of the texts come from the Cesnola collection, unscientifically made (1875-77, a wryly entertaining story) by the American consul in Cyprus, who became (1879-1904) director of the Metropolitan Museum, where the collection now is. Others are from the excavations of the University Museum, Philadelphia (1934-52). The arrangement is chronological, in four parts, with every surviving inscription illustrated.

(1) Thirty inscriptions (from the seventh to the fourth century B.C.) in the Cypriote syllabary, invented for a non-Greek language and later used for Greek. Gold bracelets and a silver bowl are inscribed as the property of seventh-century kings. Useful "grids" of various signaries are appended.

(2) Forty-seven Hellenistic inscriptions, including honors to Ptolemies, their wives, admirals, garrison commanders, and governors (honored by the priests of Apollo: religion supported the civil and military establishment).

(3) Roman Imperial (128 texts). Here again, still in Greek, the ruling power is adulated, especially if, as under Nero, Trajan, Hadrian, and the Severans, it beautified the city with buildings (indicated on plans). Honors to a monster like Commodus are recorded. There is a hymn (in bad hexameter verse) to Hadrian's boy-love, Antinous. Most interesting is a set of lead curse tablets, on which various parties to lawsuits (for example over the ownership of domestic animals) call on outlandish gods to deprive their opponents (a woman, a governor, a banker, a husband) of life, soul, and strength.

(4) Early Byzantine. Especially, in the mosaic floors of a late fourth-century annex to the

baths, eight inscriptions testifying both to the arrival of Christianity and to the survival of pagan myths (Achilles among the maidens, Zeus abducting Ganymede), and pagan virtues (Modesty, Moderation, Good Order). Twelve indexes close the work, a model scholarly publication for which sponsor and editor deserve the highest praise. The editor has done his job with a sense of proportion, a sense of humor, and a flawless competence worthy of more significant material.

One possible, though pessimistic conclusion: one way to survive as a civic entity for a thousand years is to pay unremitting lip service to the ruling power: kings of Paphos, kings of Egypt, emperors at Rome or Constantinople. In the end, your city is destroyed by an earthquake.

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MEDIEVAL

RAINER BERGER, editor. *Scientific Methods in Medieval Archaeology*. (UCLA Center for Medieval and Renaissance Studies. Contributions: Number 4.) Berkeley and Los Angeles: University of California Press. 1970. Pp. xvii, 459. \$20.00.

This book has been published under the auspices of the Center for Medieval and Renaissance Studies of the University of California and consists of twenty papers presented originally at an international conference in Los Angeles in 1967 by a number of well-known experts in the field. It covers the period A.D. 300–1600.

It starts with an introduction by Lynn White, jr., who pleads for a more conscious blending of the interests and methods of the humanist and the natural scientist. The contribution of archeology as an aid to the understanding of the classical period has been accepted for a long time, but this has not been so in the study of the Middle Ages where the documentary material is so massive. But this veritable glut tends to give a one-sided picture as the motivating agents of the period—the merchants, entrepreneurs, and craftsmen—rarely put pen to paper. The documents tend to be those of clerics and even the excavations those of churches. So the historian has to adopt some

of the techniques of the prehistorian and use the numerous and now well-tried techniques of archeology. White refers to the work of the great Merovingian archeologist Edouard Salin, of Kolchin at Novgorod, and of Biddle and others at Winchester, as examples of the contributions of archeology to the understanding of the everyday life of the people not thought worthy of record by the clerics. He refers to problems that have not been solved by documentary evidence but that are proving amenable to archeology, such as the techniques of ship construction and the advent of the nailed horseshoe. Perhaps it will be possible to apply one day the techniques of carbon-14 dating to the actual parchment and vellum documents themselves, since many of these are not accurately dated.

The first and greater part of the book is on dating methods. We have several contributions from leaders in the radiocarbon-dating field such as Libby and Suess, and naturally there is considerable emphasis on timber-framing and dendrochronology. We are given a historian's view of the role of carbon-14 dating and are treated to the view of architects and typologists on English timber-building construction. The various contributions deal with the problems and pitfalls, the need for collaboration between the physicist and the archeologist, and the reasons for discrepancies in dating. The editor himself contributes one of these papers and emphasizes the fact that disturbed stratification and mixing are much more frequent than is generally thought. Seeds are often unreliable.

The effects of sunspots and variation in cosmic-ray flux are discussed by Carey and Damon. Other effects on carbon-14 dating such as nuclear weapon testing are well known; the effect of large meteorites is thought to be small or negligible, and even the dilution of atmospheric carbon 14 by volcanic emanations is thought to be insignificant except on a local basis. The use of fossil fuels has added great quantities of inert carbon to the atmosphere, but this effect is only important since 1890.

One of the lesser-known techniques of limited application is that of tephrochronology or dating of volcanic layers in countries such as Iceland. This is rather like De Greer's varve technique. In Iceland thirty volcanoes have been active since the Norse Settlement of A.D. 870, and about 200 eruptions have occurred,

leaving their layers of tephra (ash). The main eruptions are dated and can be identified in the soil profile and thus can be used to date ruined habitations.

To complete the picture we have contributions on thermoluminescence and archeomagnetic dating that are finding increasing use. The first in the dating of pottery—unfortunately destructively—but shards and scrapings are quite sufficient. Preliminary determinations have shown that the method has good possibilities for the Renaissance and medieval periods. The technique of archeomagnetic dating has proved itself in Britain as a method of dating burnt clay structures *in situ* such as kilns and furnaces. It is particularly applicable to the early medieval and post-medieval periods; but in Britain because of the particular type of change in the magnetic direction over the period A.D. 1150–1500 it is necessary for the archaeologist to decide whether the date is before or after A.D. 1300. This limitation does not necessarily apply elsewhere, but a considerable amount of correlation work is necessary before the technique can be used outside Britain.

The second part of the book is entitled "Tracing Methods," but this is not a good description for most of the papers it contains. In this section the editor has grouped the papers concerned not with dating techniques but more with analysis. Patterson and his colleagues suggest the use of the lead content of polar snows as an indication of early industrial activity and of silver production in particular and give figures obtained on snow samples from northern Greenland covering the period A.D. 1750 to the present. Other elements are considered, such as copper and vanadium as indications of copper production and coal burning in particular.

Brill discusses the composition of Islamic lustre glasses containing thin metallic layers of copper and silver. He confirmed his conclusions by simulation experiments that showed that the transparent amber decoration was essentially silver staining and that the yellow and red were copper stains. The former are brought about by an ion-exchange mechanism. This is followed by further contributions on Near Eastern pottery and glazes and the provenancing of pottery by trace element determination using neutron activation.

Further examples are given of new analytical techniques such as a potentially portable nondestructive X-ray fluorescence technique for metal artifacts using an X-ray source and semiconductor detectors in which copper, arsenic, and silver were determined in the surface layers of a predynastic Egyptian spearhead in a time of two minutes. Other artifacts examined were a bronze mirror, which appeared to have been tinned on one side, and a late bronze clamp, which had iron rivets.

Finally we are treated to some papers on magnetic prospecting and an interesting addendum on relative corrosion rates of various metals in different environments.

As we see the greater part of the book is devoted to methods of dating and in particular the carbon-14 techniques. It therefore tends to show the contributions of the physicist rather than those of other scientific disciplines. This emphasis has become rather common, and we have had quite a number of recent books of this sort. But this may be excused to some extent when one remembers that it is this aspect that appears to have had its greatest impact on archaeologists as distinct from archaeology. The book suffers to some extent from the problem inherent in such a collection of papers—that of a certain amount of duplication of matters of principle—but far less than many other works of this type. It is difficult to know how far the editor is responsible for the value and interest of this book—the conference organizers may have been merely lucky or extremely efficient in their choice of contributors. But I feel that the editor should be given the benefit of the doubt for the result and congratulated on the production of an extremely important work of value to both the student and the specialist. The contributions are supported by references to recent papers and well illustrated with tables, diagrams, and photographs. A short index has been provided.

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D. A. BULLOUGH and R. L. STOREY, editors. *The Study of Medieval Records: Essays in Honour of Kathleen Major*. New York: Oxford University Press. 1971. Pp. xvi, 327. \$14.50.

These essays honor a distinguished former Reader in Diplomatic and later Principal of St.

Hilda's College in the University of Oxford. Their almost uniform excellence is a tribute to Miss Major's skill and inspiration as teacher and scholar.

D. A. Bullough's "The Writing-office of the Dukes of Spoleto in the Eighth Century" is a warning of how political historians can be misled by the language of protocols and eschatocols of documents. It seems out of place in this volume, since all the other contributions deal with England. Pierre Chaplais's "English Diplomatic Documents to the end of Edward III's Reign" is the first thorough study of its kind. Taken together with his recent *English Royal Documents* (1971), it is a substantial beginning to a much needed history of English diplomatic. Jane E. Sayers's "Papal Privileges for St. Albans Abbey and Its Dependencies" is primarily descriptive. In his "The Assizes of Henry II: the Texts" J. C. Holt successfully challenges the argument of Richardson and Sayles against their genuineness. G. W. S. Barrow's "The Early Charters of the Family of Kinninmonth of that Ilk" illustrates the uses to which diplomatic can be put to work out genealogy. It is as quaint as its title, since the author has a penchant for Scottish spelling (for example, *infelt*, *infeltment*). David Walker's "The Organization of Material in Medieval Cartularies" is a useful guide to what one can expect to find in cartularies. G. H. Martin's "The Registration of Deeds of Title in the Medieval Borough" is a fascinating study in conveyancing outside the rules of common law. A. A. M. Duncan in "The Making of the Declaration of Arbroath," arguing primarily from sigillography, reconstructs the timetable behind that curious document. Dorothy Owen's "The Records of the Bishop's Official at Ely" demonstrates how essential is a knowledge of administrative development if the archivist is to avoid pitfalls in cataloging ecclesiastical documents. Using case histories to discuss "The Reliability of Inquisitions as Historical Evidence," R. F. Hunnisett concludes that "from their haphazard mixture of fact, fiction, and error the complete truth can rarely be distilled." Using PRO early Chancery proceedings, R. L. Storey demonstrates how "Ecclesiastical Causes in Chancery" can supplement and correct material found in bishops' registers. The last two essays are self-descriptive: "The Privy Seal Clerks

in the Early Fifteenth Century" by A. L. Brown and "Some Archdeacons' Court Books and the Commons' Supplication against the Ordinaries of 1532" by Margaret Bowker.

Anne Whiteman has written an appreciation of Miss Major and A. E. B. Owen has compiled a bibliography of her writings.

G. P. CUTTINO

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MICHEL MOLLAT and PHILIPPE WOLFF. *Ongles bleus, Jacques et Ciompi: Les révolutions populaires en Europe aux XIV^e et XV^e siècles.* (Les grandes vagues révolutionnaires.) [Paris:] Calmann-Lévy. 1970. Pp. 328.

Messieurs Mollat and Wolff claim to have produced the first book dealing in any comprehensive way with the popular movements of the fourteenth and early fifteenth centuries—all previous studies being confined to particular local risings.

After pointing out the prime defect of their sources—that almost all contemporary accounts come from the pens of those hostile to the insurgents—the joint authors trace the change in the towns from the twelfth- and thirteenth-century movements to gain emancipation from the control of their feudal lords to the first internal struggle of the "moyens" against the patricians occurring around 1280–85, through the chronic unrest of the more developed urban centers and rural regions to the great upheavals of the years 1378–82 and the later Hussite and Taborite movements.

According to our authors, the enormous expansion of both population and wealth in the twelfth and thirteenth centuries had brought about greater inequalities both within and between the various social classes, and the more economically advanced the area the greater the inequalities. Added to the chronic discontent that this secular trend produced, the middling and lower ranks of tradesmen, artisans, and peasants fiercely resented the perennial corruption and self-seeking of urban aristocracies and rural seigneurs, seeing the upper ranks of society perverted from their true role of protectors into becoming oppressors.

Though particular risings were conditioned by chronic economic resentment, it was often political incidents or governmental pressures that sparked them. This is particularly true of

the years 1378–82—the Ciompi in Florence, conspirators in Brunswick and Lübeck, peasants in Puy, Nîmes, the Ile de France, and Normandy, and above all in England rose at first in resistance to what they regarded as unjust demands for taxation.

The authors rightly reject the Marxist contention that these social-cum-political revolts rose out of a feudal crisis complicated by the development of a growing capitalism. Instead they favor the interpretation of a Malthusian crisis due to an excessive growth of population. They also deny any genuine idea of resolution as the modern world understands it. Except for a sprinkling of fifteenth-century chiliasts, few insurgents had any idea of changing the bases of society. Many, in fact, admired the Christian ideal of holy poverty, which, after all, hardly produces a revolutionary fervor for economic growth. Most looked to some rather vague traditional ideal of good lordship as their ultimate goal.

Few figures exist as a basis for the economic history of this period, but the authors might well have made greater use of what there are. Otherwise Messrs. Mollat and Wolff have provided a most interesting contribution to late medieval history—one that should certainly stimulate readers to investigate their theme further.

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M. G. A. VALE. *English Gascony, 1399–1453: A Study of War, Government and Politics during the Later Stages of the Hundred Years' War.* (Oxford Historical Monographs.) New York: Oxford University Press, 1970. Pp. xii, 271. \$11.00.

In this book emphasis is laid on the varying importance of Guyenne in the more general context of Anglo-French relations. Much attention is given to the English war aims, but in addition there is an illuminating chapter on the Gascon nobility and the problem of its allegiance, an all too brief conclusion, and seventeen pages of appendixes. The volume is packed to the brim with information, much of it breaking new ground, and it will have to be the starting point for any further research in this field. Specially noteworthy is the handling of such questions as sovereignty in Guyenne

and the wine trade with England, but there are few aspects of his subject to which Dr. Vale does not make new additions.

Despite this, he might have written a better book if he had attempted a broader vision and had been a little less merciless with detail. He notes, for example, that already before 1453 English enterprise was turning toward Iceland and the Baltic lands, and that ships of Robert Sturmy of Bristol were in the Mediterranean in 1457, but he relates these facts to the loss of Bordeaux rather than to broad causes that had been operating over a long period. He quotes Professor Wernham's challenging generalization that the "continentalist" policy of the fourteenth and fifteenth centuries, resting on the tenure of areas within France as "barbicans," gradually gave way to the insularity of the Tudors, but he hardly gives it the critical scrutiny it deserves.

Two minor points may be mentioned. Dr. Vale examines with his usual thoroughness the question of whether or not Henry V at one time in 1415 planned a landing in Guyenne rather than Harfleur. He believes that the possibility of an early preference for Bordeaux is not as remote as some historians have thought. But in the end, it seems, the evidence only serves to suggest how determined Henry was that his destination should remain a tight secret until the very last moment. It should be added, perhaps, that few writers would have refrained, in this context, from drawing the parallel between 1415 and 1944.

In contrast to this lengthy discussion, Dr. Vale omits the decisive battles of both 1415 and 1453. No one can quarrel with this decision, considering how much he has given. But the omission still seems a little odd, in a volume whose subtitle gives as its first preoccupation a study of war.

However, within the limits he has set for himself, Dr. Vale has given us a work of real importance. He has filled a notable gap in a field that has been badly neglected by English-speaking historians for too many years.

B. WILKINSON

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PAUL M. KENDALL and VINCENT ILARDI, edited with translations by. *Dispatches with Related Documents of Milanese Ambassadors in France*

and Burgundy, 1450-1483. Volume 1, 1450-1460; Volume 2, 1460-1461. Athens: Ohio University Press. 1970; 1971. Pp. lvi, 390; xxx, 486. \$20.00 each.

In his *Renaissance Diplomacy* the late Garrett Mattingly forced us to examine in new light the interrelations of late medieval and early modern states and principalities. His sensitivity to shifting methods as medieval practice surrendered to more subtle and effective Renaissance modes has been admirably supplemented by the work of Donald Queller and recently further enhanced by the translation into English of François Ganshof's *History of International Relations for the Medieval Period*. Now in the first two of what is to be a collection of three volumes Professors Kendall and Ilardi provide rich material for those concerned with the political history and ambitions of Milan, France, Burgundy, England, and the lesser Italian powers between 1450 and 1483. The originals of the dispatches here published are usually written in Italian. There are, however, a number in French or Latin. All have been translated into English, with the translation and the original text facing each other on opposite pages. The demands of the two languages do not permit a line-for-line correspondence, but the corresponding parts of each document are easily identified by those familiar with both languages. The English version reads smoothly and where tested appears to be accurate. In this day of prohibitive costs it is comforting to encounter the luxury this arrangement affords and to note the astonishing liberality of the publisher in providing fine, clear print and generous margins. These books are a delight to handle and are indeed outstanding examples of the publisher's and the editors' respect for quality in form and matter.

The dispatches in volume 1 are often widely spaced in time and give rather scattered bits when compared with those of volume 2, which are all ascribed to the months between August 27, 1460, and July 20-21, 1461. For the most part the letters are ones exchanged between Francesco Sforza, duke of Milan, and his emissaries on foreign assignment. Their concern is primarily with problems of statecraft, and one looks usually in vain for attention to other matters. This is not surprising, for a Prospero

Camogli or a Francesco Coppini had the obligation to follow instructions, to get and to transmit information desired. Like the good intelligence agents they were, eyes and ears were always alert and few moves in their assigned areas went unreported. The frown of a prince or the sudden appearance of another agent revealed much to the perceptive observer. From attentive reading of these reports, along with the original instructions and replies, one does get insights into the characters of the writers and obtains at least the emissary's appraisal of a Charles VII in his declining years, a more favorable view of Louis XI as Dauphin than is often the case, and fluctuating and varying opinions of Philip of Burgundy. Obviously the evaluations are those of the writers of letters, save where they admittedly report those of others.

By careful reading of the dispatches one encounters numerous bits of miscellaneous information that are of more than casual interest, even if not germane to issues of high policy. Angelo Acciaiuoli, for example, in late December 1451 remarks on the French king's apprehension about a large body of restless, unemployed troops on hand. Early in 1452 he tells Dieci di Bilja that he has little to report but does not miss the opportunity to remark that he had had his hands full simply in traveling, but a few days later he is able to report the arrival at Tours of an ambassador from Aragon who is minus an ear. In a later letter he comments on the advantages of brevity, for he has doubtless been warned that prolixity in letters is not desirable. Emanuele de Iacopo writing from France in July 1460 warns Sforza not to read parts of a missive aloud in the presence of others. Similar admonitions are frequently met, and it is obvious that these fifteenth-century agents were instinctively sensitive to the elements of sound intelligence practice. Here and there one finds references to conditions of travel and the animosity of various people and nations, but only rarely of difficulties encountered en route. Is Tommaso da Rieti trying to impress Duke Francesco when he opens his letter of March 28, 1454, by announcing that it took him eleven days to go from Tours to Aix, "a distance of 440 miles, and on the road I beat the hide off several horses"? The agents fre-

quently complain of the failure of the "home office" to answer their letters. De Iacopo once exclaimed (1:336) to the duke that "this deadly uncertainty" had so upset him that "I have been unable to sleep or eat anything that would do me good." All writers of letters can appreciate the rhetorical exhortation of Coppini (2:278) when he begs Francesco to be patient with his miserable handwriting ("haver patientia con questa trista scriptura di mia mano"). In letters written in 1461 da Camogli has much to report about a meeting of the Order of the Golden Fleece at St. Omer, and in the one dated May 9 he gives full advice to the duke as to how Count Galeazzo Sforza can "make" this exclusive fraternity. He strongly advises that pressure be exerted in proper places to assure the count's election. In a later letter it is amusing to read his warning to the duke that "the Dauphin [Louis] no more keeps a secret than a wicker basket holds water" (2:444). These tantalizing bits gleaned here and there from the correspondence are offered to arouse curiosity and to serve as examples of what may be discovered in this fine collection.

The dispatches are carefully edited and provided with the notes so necessary for a full appreciation of what they give. Happily these are appended to each letter and are easily consulted. Each volume has its own index and its own introduction, with a preface in volume 1. Mr. Kendall is primarily responsible for the translation of documents and for the notes dealing with French and English affairs; Mr. Ilardi for establishing the text and for historical notes dealing with Italian affairs. They are to be congratulated for work well done. One looks forward to the completion of this series.

GRAY C. BOYCE

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R. WELLDON FINN, *The Norman Conquest and Its Effects on the Economy: 1066-86*. (Domesday Studies.) [Hamden, Conn.:] Archon Books. 1971. Pp. xiv, 322. \$16.50.

Mr. Finn has provided a survey in this book of the influence of the Norman Conquest on the English economy between 1066 and 1086. He has attempted to calculate the changes in regional and local prosperity by using the three

principal features of Domesday Book, namely, "the values of holdings, the extent to which agricultural capacity was being exploited, and the social and economic standing of small tenants and villagers." The nature of the information decrees more likely success in the first of these categories, but abundant uncertainties plague conclusions in all three.

The triple valuations (*tunc, post, modo*) of land holdings that are necessary for a well-founded appraisal of the rise and fall of values are never as numerous as desired. Frequently, only double or single figures are given. Land values tended to fall everywhere as an immediate result of the Conquest (*tunc-post*), but no figures are available for East Anglia and the northern counties in this respect. Of twenty-six counties with rise and fall figures for the 1066-86 period (*tunc-modo*), thirteen fell and thirteen rose. The rise figures mean that the counties not only recovered from the interim fall but appreciated over twenty years beyond their 1066 levels. On the other hand, the fall figures are usually larger than the rise items and suggest that destruction and damage had far greater impact than the factors of recovery. Furthermore, the 1086 rise figures often conceal overvaluation for purposes of obtaining excessive rents. Mr. Finn has improved on the early work of F. H. Baring and the more recent work of C. H. Lemmon by calculating percentages of fall and rise as a more significant index of change and the intensity of military destruction.

The "fall" counties constitute three distinct areas: Middlesex and the five counties to the north and northwest of London, which endured the last part of William's march in 1066, the March counties, and those north of the Trent. So stricken was Yorkshire that in 1086 the total land value still remained seventy-eight per cent below its pre-Conquest level.

Ploughlands generally exceeded plough teams in number, an indication of exploitation below capacity, but the author's conclusions on the peasant population are less certain. One might wish for a large-scale map showing rise and fall percentages for all of England and for more comprehensive tables on the other statistical categories. This book is a statistical study in prose, which is by nature an uncongenial

inating, but it is also a volume of absorbing interest and is written, as we would expect, with consummate care.

FRANKLIN J. PEGUES
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J. R. MADDICOTT. *Thomas of Lancaster, 1307-1322: A Study in the Reign of Edward II.* (Oxford Historical Monographs.) New York: Oxford University Press. 1970. Pp. xii, 390. \$14.50.

This is a peculiar book to review. It contains too much good research, sound judgment, and knowledge of the fourteenth century to merit censure. A reviewer, however, has an obligation to the readers as well as to the author, and I must say that I think the book has some serious flaws.

Through most of its length the book is a narrative of baronial politics in the reign of Edward II, centering around but by no means confined to the king's cousin, Thomas, earl of Lancaster. As such it takes its place, with the works of T. F. Tout and with J. C. Davies's *Baronial Opposition to Edward II* (1918) and May McKisack's *Fourteenth Century* (1959) as a major treatment of the period. Dr. Maddicott has industriously and judiciously blended the contemporary chronicles, the record sources (using the duchy of Lancaster archives as well as those of the king's government), and modern scholarship to tell his tale. He offers a solid piece of work.

But of the book as a whole I have two criticisms. One is that it is dull reading. It has a patchwork quality: the separate bits are all well done, but the seams are often weak. The first two chapters make an excellent monograph on baronial estates, finances, and retinues, but they are not integral to the main tale. Several asides, like those on the fatal desertion of Lancaster by his retainers after 1320 (pp. 295-97) and on his ecclesiastical benefaction and patronage (pp. 319-21), would make neat articles but here are awkward albeit suggestive snippets. Finally, much of the writing is poor. Authors who produce 340-page monographs should spare the reader paragraphs of 700 words.

My other criticism is that the author has been too austere. After several years of work on Earl Thomas he might well have offered us some informed guesses on matters we can never

resolve. Much of the hostility between the king and Thomas of Lancaster was personal. Therefore I would welcome Maddicott's views about his principles. If his effort to take the conflict out of the context of a struggle for administrative control is not wholly convincing, his few pages on Thomas as a later day Simon de Montfort are most intriguing. We have here a serious book. Had the manuscript been trimmed and its focus sharpened, we might have had an exciting one as well.

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J. G. BELLAMY. *The Law of Treason in England in the Later Middle Ages.* (Cambridge Studies in English Legal History.) New York: Cambridge University Press. 1970. Pp. xviii, 266. \$14.50.

The term "monograph" has been abused and extended to cover practically any form of scholarship of less than three hundred pages. Short biographies, edited diaries, and statistical compilations have all been lumped together under the utilitarian shorthand rubric of "monograph." It is a misfortune, this semantic abuse, because one is deprived of a proper term to describe precisely a work such as Professor Bellamy's. Here is the monograph in the best sense—less concerned with enumerating all counts of treason or even all curious kinds than with illuminating the maintenance of the charge and how and why it was used.

Professor Bellamy's scholarship is impeccable, and he both mechanically and intellectually separates his conclusions from this scholarship. In the same fashion, the organization of the book is particularly suitable for the intermixture of act and abstraction illuminated in medieval treason. Starting with concepts and proceeding through commentators, then early trials, to the great statute of 1352, and ending with the scope of the act and its handling by the courts, Professor Bellamy takes the reader through a totality of law for an age, and then ends with addenda chapters on attainder to tidy up the applications of the concept.

Certain of Professor Bellamy's conclusions may be arguable but largely from the stance of his not attempting to please this or that constitutionalist who would prefer some emphasis

on his own theories: I would have liked a little more explanation of the coincidence of the English formal claims to the French crown and the concern of the English for a statute covering treason (a much earlier formal definition, I believe, would have been embarrassing since under the 1352 definition the English king, while occupied with his Angevin heritage, was certainly treasonable to the king of France). Such criticism, however, could only be considered proper had Bellamy made claim to exhausting every theory, every trial, and every commentator on English treason in the late Middle Ages, which he does not and should not in this work.

This then, is an admirable volume, a monograph of the finest sort, and it upholds the high standards that the Cambridge University Press has established under the general editorship of Professor Yale for its Studies in English Legal History.

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JEAN FAVIER. *Les contribuables parisiens à la fin de la Guerre de Cent Ans: Les rôles d'impôt de 1421, 1423 et 1438*. (Centre de Recherches d'Histoire et de Philologie de la IV^e Section de l'École pratique des Hautes Études. Fifth Series, Hautes études médiévales et modernes, Number 11.) Geneva: Librairie Droz. 1970. Pp. 365.

Jean Favier, whose previous work in political and fiscal history ranges over the fourteenth and fifteenth centuries and from biography to papal finances, has now turned his talents to editing and analyzing texts. In this book he provides a meticulous edition of three tax rolls of Paris: the lengthy account of the tax levied in 1421 to reform the currency; the short record of a forced loan taken in 1423 from the richest citizens; and the longer account of the forced loan of 1438 for the defense of Saint-Denis. While the second text has once before been published, the others have not been completely edited, nor has any of them been subjected to the close scrutiny Favier gives them. The documents are carefully presented, with fewer intrusive modernizations of punctuation, capitalization, and accentuation than are often found. Two helpful indexes list and give information about the individuals named in the texts and group them by professions.

Favier's introduction, which contains seven graphs, nine sketch maps, and seventeen tables, demonstrates what *explication de texte* can be in the hands of a scholar who has mastered both statistics and philology and who is concerned with social and political as well as purely financial history. Having identified the *quartiers* of Paris in 1421 and 1438, referred to in the records only by the names of their chief officials, Favier analyzes the occupations grouped in different parts of the city and the districts' relative wealth. Academics will not be surprised to learn that the Left Bank thronged with tavern keepers and was totally devoid of money changers. Then as now the Right Bank embraced the richest and poorest sections. Using all three accounts, Favier constructs an economic hierarchy of professions and examines diversities within occupational groups. The yields and levels of assessment of the taxes are compared, and significant differences are related to the circumstances in which the levies were made. In a particularly interesting section Favier shows how patronage and status affected liability, making the taxes more regressive than they would otherwise have been. The effect of exemptions on the profitability of the tax of 1421 is described in detail.

Favier has prepared an informative and useful book. It should persuade confirmed skeptics that, even in the day of microfilm and xerox, the careful editing of such texts as these has great value.

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ANDREW SHARF. *Byzantine Jewry: From Justinian to the Fourth Crusade*. (The Littman Library of Jewish Civilization.) New York: Schocken Books. 1971. Pp. xiv, 239. \$7.50.

Scholars and students of the medieval Greek world and of medieval Judaism will welcome the present volume with a feeling of gratitude. This is a substantial contribution and fills a real need in Byzantine Jewish studies, for no synthetic account of Byzantine Jewry had been attempted since 1914, when Samuel Krauss published his *Studien zur byzantinisch-jüdischen Geschichte*.

In eleven chapters and two appendixes Dr. Sharf, professor of history at Bar-Ilan Univer-

sity, Jerusalem, gives a thorough and clear picture of the Jewish community in the Greek Christian East from 527 to 1204. Careful scholarship and objective and critical use of the sources characterize this work, which attends to the concrete facts.

In the course of seven centuries there were Jewish communities in more than twenty cities of the Empire including Constantinople. To be sure there is anti-Semitism in the writings of certain Church fathers and in liturgical books, and there were laws and canons discriminatory against the Jews. And occasionally Jews became the scapegoat when the Christian population tried to explain an epidemic or a natural catastrophe. But much of patristic or liturgical anti-Semitism was rhetoric, and neither Church nor state ever emphasized what had been legislated either by Heraclios, by the Council in Trullo, or by other emperors. The author demonstrates convincingly that the Jews were an integral part of the citizenry and had identified the Empire as their home and country (p. 74). When decrees or canons were issued against religious minorities, including the Jews, they were issued in order to safeguard the security of the Empire and to reinforce imperial authority by eliminating the dangers of religious dissent. Thus anti-Semitic decrees were seldom enforced and their application and consequences were extremely limited (pp. 71, 102). When because of occasional outbursts the Jews suffered, usually they suffered "for their participation in some general movement against the authorities" (pp. 44-48, 53-54, 56).

Contact between Jew and non-Jew was close if not necessarily friendly; business with Jews was normal, if not welcomed. The Jews were not isolated from the issues that concerned the interests of the state (pp. 117-18), and they enjoyed the same freedom of movement that all citizens enjoyed within the Empire (p. 116). A Jew in his city could say without any fear "I am a Jew." Professor Sharf demonstrates repeatedly that the Jews fared much better in the Greek Christian East, enjoying conditions there superior to those in Western Europe (pp. 163, 198).

At times I wish the author had been more analytical. I wonder to what degree hagiographical texts illuminate the life of the Jews

in the country or in small towns. Two more critical points. I believe that the author errs in speaking about "the mass immigration of Slavs and their very recent conversion to Christianity" in the seventh century. The conversion of the Slavic settlements in the lower Balkan peninsula took place during and after the reign of Irene. As a modern authority on the Slavs writes: "The Slavs in the early decades of the ninth century . . . were . . . no doubt still pagan" (A. P. Vlasto, *The Entry of the Slavs into Christendom* [Cambridge, 1970], 10-12). Also I doubt whether the sources bear him out in his evaluation of Romanos Lecapenos (p. 97).

Apart from these critical observations, I find that the present volume is very valuable to historians, theologians, and the educated layman as well.

DEMETRIOS J. CONSTANTELOS
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FRANCIS DVORNIK. *Byzantine Missions among the Slavs: SS. Constantine-Cyril and Methodius*. (Rutgers Byzantine Series.) New Brunswick: Rutgers University Press. 1970. Pp. xviii, 484. \$17.50.

Through four and one-half decades Father Dvornik's name has been inseparably connected with the crucial question of the Christianization of the Slavs. Now in a handsomely presented volume in the Rutgers Byzantine series, he has given us a masterful narration and analysis of the Byzantine role in this process. The nine chapters of the book are devoted first to early Byzantine, Roman, and Frankish missions among the southern Slavs and to the first contacts with the Russians. Next, extended treatment is given to Moravia both before and during the mission of SS. Constantine (Cyril) and Methodius; to Rome's relationship with this mission; and to Methodius's work after his brother's premature death. Finally, Dvornik deals seriatim with the Cyrilo-Methodian heritage in Poland, Bohemia, Croatia, Bulgaria, Serbia, and Kievan Russia.

Two related themes are woven subtly through this study. One, arising partly from the Byzantine vantage point that Dvornik takes, is the debt the several Slavic peoples owe Byzantium. Either directly or indirectly the

eastern heir of Rome provided religion, legal and political institutions, writing, and literature to its cultural offspring. The second theme is the unique character that Byzantine missions developed in this area. Despite attempts at Hellenization, there grew up a distinctive Slavonic tradition, composed of both language and literature, which could be traced back to the Cyrilo-Methodian achievement. In Dvornik's view, the rejection of this tradition by the Latin church greatly hampered its efforts among the Slavs. Conversely, its cultivation by the Byzantine church facilitated the acceptance and development of an eastern cultural and religious atmosphere. Only among the western Slavs was the Cyrilo-Methodian heritage successfully overlain, though not wholly forgotten, by both Roman and Germanic religious and political activity.

Among Dvornik's many important books, two in particular (*Les Slaves, Byzance et Rome au IX^e siècle* and *Les légendes de Constantin et de Méthode vues de Byzance*) have been devoted to material that forms the core of this book. It is appropriate, therefore, to ask what new contributions are to be found here. In addition to sharpening and clarifying many lesser points, one major achievement is Dvornik's synthesis of the archeological evidence that has accumulated in Central and Southern Europe in the last two decades. From this he provides a far clearer picture of the social, religious, and even political structure of Moravia before and during the Byzantine mission. In this regard, the illustrations (seventeen floor plans of recently unearthed churches and nineteen photographs of related artifacts) greatly add to the value of this book. In addition, this book now provides a clear, reliable treatment of the Slavonic rite in Poland and Bohemia and its probable eastward influence. Much of the remaining material of the book, however, does not depart sharply from or advance significantly beyond what Dvornik and others have already told us. Nevertheless, by its comprehensive treatment of Byzantine missions to the Slavs and its careful analysis of the views of European scholars, the book will rightly be welcomed as a major study, sure to become a standard in the field.

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PAUL LEMERLE *et al.*, editors. *Actes de Lavra*. Part 1, *Des origines à 1204*. Volume 1, *Texte*; Volume 2, *Album*. (Archives de l'Athos, Number 5.) Paris: Éditions P. Lethielleux. 1970. Pp. x, 447; 80 plates. 390 fr. (\$71.00).

This new diplomatic edition of the Acts of the Grand Lavra replaces, completes, and corrects that of Germaine Rouillard and Paul Collomp, of which only one volume appeared in 1937. Rouillard and Collomp edited fifty-seven documents dating from before the inception of the monastery until 1178 (a date without significance), while Lemerle and his collaborators are now offering us sixty-nine, down to 1204, plus an appendix containing six texts of doubtful authenticity or not directly relevant to Lavra. Of the sixty-nine documents, twelve (nos. 13, 15, 20, 22, 25-27, 59, 60, 65, 67, 68) have not been previously published, one (no. 16), unknown to Rouillard and Collomp, was nevertheless edited by Porfirij Uspenskij in 1880, and one (no. 53) by André Guillou in 1957; the rest are in Rouillard and Collomp, though often in a substantially less correct form. As regards the appendix, two documents are in Rouillard and Collomp, the remaining four being published here for the first time.

The new edition has been long in the making. The manifold obstacles which the monks of the Holy Mountain persist in placing in the path of scholars, the death of the two original editors, and the dislocation caused by the Second World War have all been responsible for the delay. What is more disturbing, however, is the recent loss of several important documents. Numbers 2 (deed of sale of A.D. 941), 4 (judicial decision of 952), 11 (fiscal document of 994), 44 (chrysobull of Alexius I of 1082), and 48 (chrysobull of the same emperor of 1086), most of which were still preserved in 1944 and 1945, could no longer be traced. The cartularies of the monks Alexander and Kornelios have likewise vanished. One cannot help wishing that the safeguarding of these precious archives be placed in more responsible hands.

The attention of specialists will naturally be directed in the first instance to the documents that are edited here for the first time. Three of these (nos. 15, 26, 27) concern the Athonite monastery of Bouleuteria, which Lavra managed to annex in a fairly unscrupulous fashion

in 1030. Numbers 20, 22, 25, and 60 are deeds of donation of various properties on the island of Skyros and on the mainland. Number 59, unfortunately mutilated, concerns the division in 1110 between three brothers of property situated in and around Thessalonica, and it contains valuable topographical detail. Number 65 (A.D. 1181), bearing on the same topic as the previously known numbers 66 and 69, is of the highest interest: it relates to a dispute between Lavra, which owned estates in the province of Moglena (northwest of Thessalonica), and the Cuman *pronoïars* settled in that region. The document lists the names of the Cuman squires who had usurped the services of the serfs (*paroikoi*) belonging to Lavra, as well as of the serfs themselves, sixty-two in all, many of whom were clearly Slavs. The government ruled in favor of Lavra, and the Cumans were ordered to move elsewhere. Finally, numbers 67 and 68, both of 1196, concern a judicial action by virtue of which Lavra was allowed to transport wine on its ships to Constantinople without paying duty.

It is impossible in this space to give even a faint idea of the wealth of information that the Acts of Lavra provide on the social, economic, military, ethnic, and, of course, ecclesiastic aspects of Byzantine history. Nor could the editors themselves have commented exhaustively on such a variety of topics. They have gone, however, a long way toward orienting the reader: in addition to the analysis and notes accompanying each document, there is an extensive chapter on the chronology of Lavra until 1204 by Lemerle and an illuminating essay by Nicolas Svoronos on the monastery's landholdings. In all respects, this edition is a notable achievement and does the highest credit to Professor Lemerle's center of Byzantine studies.

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GEORGES CHARACHIDZÉ. *Introduction à l'étude de la féodalité géorgienne* (Le code de Georges le Brillant). (Centre de Recherches d'Histoire et de Philologie de la IV^e Section de l'École pratique des Hautes Études. Fourth Series, Hautes études islamiques et orientales d'histoire comparée, Number 1.) Geneva: Librairie Droz. 1971. Pp. 162.

In seven of its ten chapters (2–6, 9, 10), this book deals directly and aptly with the code of George the Illustrious, King of Georgia (1314–46): its editions, translations, date, and sources; the locality it concerns; the questions of property and the *Wergeld*, and of the local administration mentioned in it; and the policies of King George and the *raison d'être* of the code. The book presents a new French translation of the code, far superior to the earlier versions in Russian, English, and (Karst's whimsical) French.

On the other hand, chapters 1, 7, and 8 deal with vaster problems than the code: the nature of Georgian feudalism and the Georgian social structure. Here, alongside proofs of much erudition of a legal and linguistic nature, there are manifestations of a fundamental historical misapprehension that it is the reviewer's duty to note.

The author wisely reacts against the Marxists' abuse, or too broad a use, of the term "feudalism," only to fall into the opposite extreme of restricting the phenomenon so designated to the territory "entre le Rhin et la Loire" (on page 10 and elsewhere). Yet the existence of feudalism in Japan is admitted and, what is more, clearly proved in Georgia. Had the pertinent works of Adontz been consulted, its existence in Armenia too would have been admitted. A total lack of conversancy with English publications (save Wardrop's translation of the code) prevented the author from profiting from the thoughtful *Feudalism in History* (ed. R. Coulborn [1956]), which avoids both the Marxist Scylla and the Charybdis of the French restrictionists. Nevertheless, the author understands feudalism, as when he distinguishes (pp. 12–13) between the struggle within that system and one between it and another system, for example, *étatisme*. It is Georgian feudalism that he seems to fail to understand.

Only a few salient points can be singled out here. One is the translation of *qrma(y)*, later *qma*. The word has three meanings: child, boy (*pais-puer*); knight; serf (a later meaning) (see Nicholas Marr and Maurice Brière, *La Langue géorgienne* [1931], 692). In earliest translations it rendered *brephos* and *pais* (Joseph Molitor, *Altgeorgisches Glossar* [1952], 192). The author himself on pages 24 and 35 states that it can mean "retainer," "vassal." Yet he translates it

throughout as *esclave* and equates it with *mona(y)* (p. 15), which is found but once in the sense of "vassal," as used by an ultraroyalist historian anxious to exalt the Crown at the expense of the nobility (see my *Studies in Christian Caucasian History* [1963], 258). It can usually be translated, indeed, as "slave." But even *mona(y)*, rendering in earliest translations (*Altgeorgisches Glossar*, p. 119) *doulos-servus* and also *pais*, need not always mean "slave," signifying also "servant," that is, *famulus*. The same ambiguity, by the way, marks also *pais* and *puer*, as well as *servus*. In view of this latitude, the choice of "slave" instead of "vassal" is inadmissible in the context of history, that is, of the ethos of a feudal nobiliary society.

More serious is the translation throughout of *eristcav(i)* as "prince" instead of as "(feudal) duke." Here a perusal of my *Studies* might have instilled an awareness, totally lacking in the book, of the dichotomous nature of the Georgian (and Armenian) nobility, at once "dynasticist" and feudal, and of feudalism (especially the dukedoms) as introduced by the Crown to control the dynastic princes, from whose midst it had itself arisen. It might have prevented, too, the identification of *eristcav(i)* with *pitiaxš(i)*.

In fine, this is a splendid study and translation of the code, but hardly a satisfactory introduction to the study of Georgian feudalism.

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MODERN EUROPE

LOUIS B. WRIGHT. *Gold, Glory, and the Gospel: The Adventurous Lives and Times of the Renaissance Explorers*. New York: Atheneum. 1970. Pp. xvi, 362. \$10.00.

Despite Dr. Wright's apparent eagerness to explain why he, an eminent specialist in English colonial expansion, should have undertaken to survey the creation of the Iberian empires, readers may still be left wondering.

At various points in both the preface and the introduction the author gives some three or four different reasons for producing his book. On page vii of the introduction, for instance, he is interested in the explorers' motivations: "what made them do it?" Unfortunately, the

naiveté of trying to explain a great historical movement via the examination of individual psychology produces results with little to recommend them, as a comparison of Dr. Wright's "explanations" with those of Pierre Chaunu at the close of his recent *Clio* volume on European expansion will make clear. Further on, the author admits to hoping that his book has "distilled the essence from learned works and made it more readily available" (page vii of the introduction). But although the book lacks a bibliography and the footnotes are few, the fact that the author evidently reads neither Spanish nor Portuguese (nor French?) means that the illuminating insights of Vitorino M. Godinho, Charles Verlinden, Pierre Chaunu, Richard Konetzke, and many other scholars, whose recent contributions have remade the field, are not reflected in these pages. Rather, it seems that basic reliance has been placed on a few old war horses, all in English: Morison for Columbus, Henry Hart (!) and (probably) Prestage for Portuguese expansion, plus copious draughts from the Hakluyt Society translations.

The actual result comes closer to fulfilling a third of Dr. Wright's professed motives for writing: "to relate important and dramatic episodes that are typical of expansion overseas. . ." (page xvi of the introduction). But here the problem is that something very close to this is already available in either J. H. Parry's much superior *Age of Reconnaissance* (1965), or to a large degree, in the old but still useful survey of F. A. Kirkpatrick, *The Spanish Conquistadors* (1934) (not to mention Bores Penrose's work on Renaissance exploration), to which Dr. Wright has little, if anything, to add.

The "dramatic episodes" around which the author elaborates his work are twelve or so in number, depending upon one's definition of "episode." There is the requisite chapter on Henry the Navigator, followed by others on the Portuguese in Africa, Vasco da Gama, Columbus, and so forth—in short, the whole moldy lot up to and including Cortes and Drake. Certainly the best and most interesting parts of the book are the last two chapters where the author is on home ground: his essay on the struggle of the non-Iberian powers to breach the peninsular overseas monopoly and his final chapter on the use of Protestant ideology to support much of the later non-Iberian

expansion are excellent. The writing throughout is smooth, the style agreeable, and the facts usually correct. In other words, the author tells a good "story"; and, blessing of blessings, he has spelled the Spanish and Portuguese names and places correctly.

Nonetheless, the simplistic, common-sense motivations beyond which the author evidently cannot see will tend to limit his audience to eager high schoolers, undergraduates of modest intellect, and that part of the general public that wants to know "what happened" without running the risk of brain strain. To professional historians, their disciples, and others possessed of a certain economic and sociological sophistication it has little to say. Gold or glory or the gospel may have been among the conscious motivations of some of the Renaissance explorers; but precisely what motivated the author to retell these too often told tales must still remain something of a mystery.

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C. G. CRUICKSHANK. *The English Occupation of Tournai, 1513-1519*. New York: Oxford University Press. 1971. Pp. xi, 301. \$10.50.

In his *Army Royal: Henry VIII's Invasion of France 1513* (*AHR*, 75 [1969-70]: 838-39), Dr. Cruickshank described Henry VIII's farcical expedition that resulted in the capture of the city of Tournai. Now he has provided us with a sequel in which he narrates Henry's five and a half year attempt to integrate this city of twenty thousand Frenchmen into the empire of England.

Henry VIII's decision to occupy Tournai in September 1513 committed England to the governance and protection of a city situated some seventy miles south of the English-held Pale of Calais. From the start Henry endeavored to make loyal English subjects of the Tournaisiens. Against the wishes of his advisers he pursued a policy of conciliation. Tournai, for example, was allowed to send representatives to Parliament in early 1514—a privilege not extended to Calais until 1535-36, nearly 190 years after its capture by the English. Henry's benevolent attitude was taken as a sign of weakness by the Tournaisiens, who, with few exceptions, refused to cooperate with the English government and spent much of their time

demanding that Henry protect their privileges as he had promised to do in 1513. The citizens' obstructionist activities coupled with the vacillating and often contradictory policies directed from Westminster placed the various English governors of Tournai in a frustrating situation. The primary concern of the governor and Westminster, though, was the military security of the city. Tournai's exposed position caused the English to spend approximately £230,000 on the garrison and construction of a citadel. Supporting the currently held view that Henry gained nothing from the Tournai venture, Cruickshank estimates that Henry lost at least £100,000 on the occupation. Burdened with the high cost of a city providing no political or economic advantage, Henry and Wolsey finally unloaded it on Francis I in 1519.

Although Cruickshank presents little that is new concerning Tournai's place in Europe's politico-diplomatic machinations, he does detail the events of the occupation. He is at his best when he discusses the occupation's military aspects. Because of the loss of much of the Tournai archives in 1940, Cruickshank has been forced to rely on previously published documents, a number of which he has checked against manuscripts in England. Unfortunately, the value of his comments on Tournai's role in European affairs is diminished because he does not seem to have examined the extant manuscripts in France and Italy.

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NARCISO NADA, editor. *Le relazioni diplomatiche fra l'Austria e il Regno di Sardegna*. First Series: 1814-1830. Volume 3 (2 settembre 1822-24 luglio 1830). (Fonti per la storia d'Italia. Documenti per la storia delle relazioni diplomatiche fra le grandi potenze europee e gli Stati italiani, 1814-1860. Part 2, Documenti esteri.) Rome: Istituto Storico Italiano per l'Età Moderna e Contemporanea. 1970. Pp. xiv, 405. L. 5,500.

The documents in this volume cover the period between the beginning of the Congress of Verona and the eve of the revolution of 1830 in Paris. The dispatches from Vienna were all signed by Metternich. Those from Turin were written first of all by Baron Daiser and then by Count Rudolf von Lützow, who was sent to the Piedmontese capital as minister plenipoten-

tiary in September 1823 and finally by Count Ludwig von Senfft-Pilsach, who replaced Lützow early in 1826.

The reader of the reports in this particular volume will perhaps not find them as interesting as those in volume 2 of the series, which deals largely with the Neapolitan and Sardinian revolutions of 1820–21 and the reaction of the Austrian government to them. Metternich's dispatches, however, do give valuable insight into Austrian diplomatic policies in the 1820s. The reports from Turin touch upon such matters as the condition of Sardinian finances and education, Victor Emmanuel I's marked anti-Austrian and Charles Felix's strong pro-Austrian proclivities, Charles Felix's reluctance to accept Charles Albert as the rightful heir to the Sardinian throne, the dangers to Sardinia and Austria of the machinations of revolutionary exiles in Switzerland, and the activities of secret societies in Sardinia. They reveal the Austrian government's satisfaction with Charles Felix, in contrast to its aversion for Victor Emmanuel I, as well as Austria's fears of French efforts to stir up an anti-Austrian feeling in Sardinia and the development of an anti-Austrian attitude among influential circles in Piedmont in spite of the painstaking efforts of Habsburg officials to refrain from meddling in the internal affairs of Sardinia.

R. JOHN RATH
Rice University

HOLGER HJELHOLT. *Great Britain, the Danish-German Conflict and the Danish Succession, 1850–1852: From the London Protocol to the Treaty of London (the 2nd of August 1850 and the 8th of May 1852)*. (Kongelige Danske Videnskabernes Selskab, Historisk-filosofiske Meddelelser, Number 45, Part 1.) Copenhagen: Munksgaard, 1971. Pp. 323. 90 D. kr.

Frederick VII being without an heir, the Danish authorities fastened upon Prince Christian of Glücksburg (the future Christian IX) as the most desirable of the eligible candidates for the throne. The diplomatic negotiations by which this choice won recognition by the European concert provide the central theme of this book. It is a theme, however, often overshadowed by weightier and far from tangential issues, for the question immediately arose: succession to what? There was the Kingdom of Denmark, of

course. But the Danish crown also carried with it sovereignty over two duchies, Holstein and Lauenburg, which were members of the German Confederation (whatever that meant before the agreement at Olmütz), and one duchy, Schleswig, whose constitutional position was highly debatable. Hence, from the start, Prussia and Austria, as leaders of the German Confederation, were involved, as was the Confederation as a whole. This, in turn, implicated Britain, as guarantor of the London Protocol, and Russia, as Denmark's protector against Prussia. Even France could not be left out of account. Exchanges between all these powers are dealt with by Hjelholt. If he concentrates on Britain's role it is because he sees this book as a continuation of his earlier *British Mediation in the Danish-German Conflict 1848–1850* (pt. 1, 1965; pt. 2, 1966; reviewed in *AHR*, 72 [1966–67]: 192, 1395).

The treatment is narrowly diplomatic, based almost entirely on the dispatches and correspondence of the diplomats involved. Background information and analysis emerge only to the extent that they do so from the diplomatic exchanges themselves. Let no one take up this book who is not well versed in Danish politics, German affairs, and the diplomatic scene generally of the time. What emerges is that Palmerston (and his successors, Granville and Malmesbury) were intent on limiting Britain's commitments on the Continent, while fending off Victoria and Albert, who wanted Britain to back the cause of German nationalism in the duchies. It is not a surprising picture, and it is certainly less contentious than Hjelholt's interpretation of Palmerston as mildly anti-Danish in his *British Mediation*.

While the book is generally well produced, its editing falls short of the ideal. Particularly annoying is the frequent absence of quotation marks where the wording suggests a direct quote. The reader can never be quite sure.

G. M. SCHWARZ
Memorial University
of Newfoundland

SIEGFRIED BAHNE, edited and annotated by. *Origines et débuts des partis communistes des pays latins (1919–1923)*. (Internationaal Instituut voor Sociale Geschiedenis, Amsterdam. Archives de Jules Humbert-Droz, Number 1.)

Dordrecht: D. Reidel Publishing Company. 1970. Pp. xliii, 655. 140 gls.

The extraordinary interest of these documents lies in the light they shed on the early difficulties of important Communist parties in the West. Their comparative cohesion, their strong innovative role, and their importance in the 1930s and 1940s, as in our own time, are all the more remarkable in view of what this archive tells us of the "infantile disorders" of their beginnings. Jules Humbert-Droz, that unusual Swiss who represented the Communist International in dealing with these parties, managed to preserve, via a mail drop in Berlin to his mother in Neufchatel, these precious records of his correspondence, reports, complaints, insights, and asides to Zinoviev, then chief of the CI. Most of them were also sent to Trotsky, whose special interest was France. Humbert-Droz's mission to these parties came at a time when the International had shifted to a "united front" line. Most of the parties involved (and there are references here to the Portuguese and the Belgian as well as the French, Italian, and Spanish) had originated, however, in the first flush of enthusiasm following the October Revolution and were pervaded by a fundamentalist faith. Mirrored in these documents was the acute difficulty of adjustment to tactics of alliance, even compromise, with the Social-Democratic movement; this was made more complicated by the heavy component of anarcho-syndicalist leaders who came to communism, the extreme factionalism, and the comparative democracy within the parties at that time that permitted groups and tendencies and gave them a freedom of expression, soon to be repressed with the onset of Stalin's "Bolshevization." One gets a sense here of the seriousness with which the communists tried to build a "world party" and the shoals of discipline and national self-assertion on which the ambitious experiment was bound to shipwreck. Humbert-Droz moved with amazing freedom from Switzerland to France and Italy. So did associates who figure in the enterprise, such as Dmitri Manuilski, later so important as *éminence grise* in Russian and Comintern affairs. In addition to the intraparty documents and the messages to and from Moscow, this volume contains attempted and fascinating definitions of the postwar period by Bukharin,

Radek, and Trotsky that suggest with what difficulty the Bolsheviks were adjusting themselves to the obvious recession of the revolutionary tide. Among those who figure in the communications is Matyas Rakosi, later the ill-starred leader of Hungarian communism. Siegfried Bahne has made an immense effort to give scholarly details in the footnotes on all the *dramatis personae*, even the most minor. The men who were later to play important roles in French communism, such as Gabriel Peri and Paul Vaillant-Couturier, are here as young men; so are Giuseppe Berti and Umberto Terracini, in the Italian case, and a wide group of figures who left communism early and later appeared in other political formations. This book is thus a mine of data hardly assembled elsewhere. As always, in looking backward, one is struck (and depressed) by the number of men and women of idealism and devotion—Russian, Polish, German, and others—who perished within the next decade and a half in Stalin's purges.

JOSEPH R. STAROBIN
Glendon College,
York University

MÁRIA ORMOS. *Franciaország és a keleti biztonság 1931–1936* [France and Eastern Security, 1931–1936]. Budapest: Akadémiai Kiadó. 1969. Pp. 453. 84 F.

The imaginative, scholarly papers submitted by Hungarian Marxist historians at the International Congress of Historical Sciences in Moscow won the praise and admiration of a number of Western delegates. This work by a young Hungarian historian, though not without its flaws, is a further demonstration that some East European scholars have broadened their horizons and are making important contributions to our understanding of modern European history.

French diplomacy clearly occupies center stage in Miss Ormos's narrative, although the geographical focus is East Europe, particularly the Danubian area. The complex search for stability in Europe, from Briand's proposal for a European Union to the abortive plans for a Danubian Pact, is here traced in great detail and with admirable skill. The role of France, on whom the responsibility for achieving "security" in Eastern Europe devolved, is closely

analyzed by the author. She provides some important new insights into the political attitudes and aims of French leaders in this period. Bartheou is depicted not as the pursuer of chimerical goals, but as a clever realist whose plan for an Eastern pact was merely a maneuver that would enable him to achieve his real goal: a Franco-Polish-Russian pact or a Franco-Soviet military convention. But the author's treatment of Soviet and British policy is less successful. Her over-harsh indictment of London's "appeasement" policy fails to take into account the military weakness and public pacifist sentiment that severely limited the options of British leaders. The discussion of the role of the Soviet Union in these events is regrettably brief and uncritical, though relatively free of the polemical tone that still tends to dominate Soviet historical writing.

The author's selection of source materials is also not beyond criticism. Too often she bases her narrative narrowly on Hungarian archival documents when relevant monographs of Western historians could have been used to advantage. It is particularly to be regretted that although four volumes of the *Documents diplomatiques français* are cited in the annotated bibliography, only two of the volumes appear to have been used in researching the work.

Despite these shortcomings, Miss Ormos's study is an important and valuable addition to our understanding of the interwar period. The language barrier unfortunately will prevent her work from reaching the larger audience it deserves, although it should be noted that a four-page French summary of the author's thesis appears in an appendix to the book, and the chapter on the Danubian Pact is available in French translation in the 1968 issue of the Hungarian journal *Acta Historica*.

THOMAS L. SAKMYSTER
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JOAN REES. *Fulke Greville, Lord Brooke, 1554-1628: A Critical Biography*. Berkeley and Los Angeles: University of California Press. 1971. Pp. xiv, 238. \$8.50.

At the outset it must be said that Dr. Rees's book will be of greater use to students of English literature than to students of history. This is owing partly to the author's desire to write a critical analysis of Greville's poetry and partly

to the fact that little material remains upon which a full-scale biography could be based. The end result, however, is disappointing. Greville always considered literature a poor substitute for action, and he himself turned to writing only when he was out of political office or out of favor at court. Since Greville preferred action to writing, it would have been better, if possible, to portray him in action. Unfortunately, even a simple chronology of his life is difficult to reconstruct. Almost all of his literary efforts were published posthumously, and there is little internal or external evidence to guide the biographer in even determining the order or circumstances of their composition.

Denied an active role in politics throughout most of his life, and Greville blames Sir Robert Cecil for this, he had much time for writing as well as a great gift for it. But he never thought of his own work or of literature in general except as an adjunct to or substitute for action. His supreme concern was that fallen mankind can be redeemed both by the Grace of God through faith and by living a pure and exemplary life in this world. The highest calling in this world was government service. If he was denied the opportunity to practice the arts of government, then he would use his pen to tell the world what the realities of politics were and how the good men should act in government.

His greatest prose work was the *Life of Sir Philip Sidney*. It was Sidney whom he considered to be the perfect embodiment of his ideal—Sidney the man of affairs, not the poet. The deeply religious and poetic soul devoting his life to politics and the affairs of men was what Greville wanted to be. Sidney's early and tragic death and his own difficulties in securing office left him no alternative but to achieve the ideal by writing about Sidney.

If Dr. Rees had been able to do more with Greville, the man of affairs, we would have a better chance to see how realistic this ideal was in the Elizabethan and Jacobin ages.

STUART E. PRALL
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City University of New York

VINCENT PONKO, JR. *The Privy Council and the Spirit of Elizabethan Economic Management, 1558-1603*. (Transactions of the American

Philosophical Society, New Series, Volume 58, Part 4.) Philadelphia: the Society. 1968. Pp. 63. \$2.00.

MICHAEL BARRACLOUGH PULMAN. *The Elizabethan Privy Council in the Fifteen-seventies*. Berkeley and Los Angeles: University of California Press. 1971. Pp. 279. \$11.50.

It is a happy circumstance for the historian when two monographs focusing on the same general problem area are published simultaneously. Professors Ponko and Pulman have provided that situation in producing finely crafted works on the Elizabethan Privy Council from differing points of interest. Fortunately, despite their diverse objectives, there is a sufficient degree of commonness in purpose, use of evidence, and ultimate conclusions to make for a reasonable comparison of their accomplishments. Vincent Ponko introduces his study of the council's role in national economic management by repeating in edited form his earlier published analysis of the work of N. S. B. Gras. The body of the work is divided into four major areas of administration and is directed at analyzing the actualities of conciliar action without entering into discussion of legitimizing moral or constitutional rationales for such action. Of these four substantive divisions of the volume, that dealing with the council's role in regulating industry and domestic trade is superior. The division relating to public and private finance is a perceptive discussion of a complex subject matter. Chapters 2 and 4, concerned with national food supply and foreign trade regulation respectively, are sound although too brief.

Michael Pulman's work, a detailed study of the executive function of the council in a drastically limited time period, while not ignoring the flow of political and institutional history, attempts to explain the *why* rather than the *what* of the council's actions. In this he is successful to the point that many of his colleagues will gratefully receive his work. His most important contribution, however, is found in parts 2 and 3, which dissect the positive and negative aspects of the council's executive function—nine chapters of rewarding reading for even advanced scholars in the field.

Together, these authors display a stimulating and imaginative use of the historian's skills and tools. And while their insights into and analysis of historical situations are penetrating

and exciting, the conclusions they draw from them are never extravagant. Because of the scholarly thoroughness of their research, students seeking materials in the history of the council will prosper—and their satisfaction will increase because of the wealth of supplementary explanatory comments in the volumes' meaty and generous citations. These, then, are works of commendable merit and contribution by knowledgeable Tudor scholars, and whatever shortcomings must be mentioned are minor in comparison with their donative qualities. Of some concern is Dr. Ponko's failure to devote more energy to the council's role in industrial wage controls. From his association with Robert Steele's *Bibliography* (1910) he is certainly aware of the abundance of Elizabethan wage regulating proclamations available. This work also presents no formal bibliography and an inadequate index. The editorial aids in Pulman's work are precise and helpful. However, he is prone to an occasional lapse in taste, as in his statement that Francis Knollys "was something of an old woman." He also tends to stretch sentences to the point of stylistic agony; they sometimes run to more than a hundred words.

PAUL L. HUGHES
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HANS P. KRAUS. *Sir Francis Drake: A Pictorial Biography*. With an historical introduction by DAVID W. WATERS and RICHARD BOULIND and a detailed catalogue of the author's collection. Amsterdam: N. Israel. 1970. Pp. viii, 236. \$80.00.

H. P. KRAUS. *On Book Collecting: The Story of My Drake Library*. (The James Ford Bell Lectures, Number 6.) [Minneapolis:] Associates of the James Ford Bell Library. 1969. Pp. 18.

A magnificent collection of source material on the life and career of Sir Francis Drake, brought together by the New York book seller and collector, Hans P. Kraus, is comprehensively described in this handsome folio volume published by Nico Israel in Amsterdam. Many of the rarest and most significant documents are reproduced in facsimile. In some cases, but not all, documents in foreign languages are translated. Contemporary maps are beautifully reprinted in foldout pages. The annotations and the introduction provide most of the

known information about Sir Francis Drake. In short, this volume is a valuable contribution to historical knowledge, for it assembles between the covers of one book material that would require diligent search in more than one library.

The story of its collection is succinctly related in a lecture delivered by Kraus at the James Ford Bell Library at the University of Minnesota in 1969. The moral of the lecture, as the author states, is that "even as late as the third quarter of the twentieth century it is still possible to assemble an important collection."

Historians will have reason to thank the collector for making so much rare source material available, for Kraus has tried to assemble everything that even tangentially touched on the career of Queen Elizabeth's most famous seaman. For example, he has included many Spanish documents that reveal the attitude of Drake's antagonists toward him and his government. These include manuscript letters, dispatches, and instructions, some hitherto unknown or neglected. For example, one of the last items in the volume is a portion of a letter from Andres Armenteros to the duke of Medina Sidonia, who had led the Armada against England, telling of Drake's death in 1596 in the Caribbean and the shipment of his body back to England in a barrel of beer. Actually he was buried at sea in a leaden coffin. Some of the maps and views dramatize historical events in which Drake was concerned. For example, included is Baptista Boazio's view of St. Augustine, the earliest engraving of any city in what is now the United States, showing Drake's forces attacking the city in 1586. The view, published in London in 1589, includes in one corner a fish, the first published representation of one of John White's drawings made when he was a member of Raleigh's colony on Roanoke Island.

The long historical introduction naturally makes Drake the hero of the narrative. Most of the praise is deserved, but the authors go overboard in exonerating the great seaman from faults. For instance, when, contrary to orders, Drake turned back at night during the Armada fight in the hope of picking off prizes, they make it appear (p. 25) that he was following a tactical plan to circumvent the Spanish fleet. Actually, he was after one of the large galleons and had the good fortune to capture the *Nues-*

tra Señora del Rosario, "the richest [prize] taken in the Armada campaign," and "a magnificent example of Drake's luck." Had Drake's conditioned appetite for treasure ships lost the fight, he might have gone down in history as something less than a hero.

Historians may also question over-romanticizing English exploits in the Armada fight, described on page 142 as "one of England's greatest victories and one of the world's decisive battles, . . . 'beginning of the end' of Spain as the dominant nation in Europe, and the beginning of English ascendancy." Of course that has been the traditional interpretation, but since Garrett Mattingly's definitive *Armada* historians have known better. In the last work that Mattingly wrote, *The "Invincible" Armada and Elizabethan England* (Folger Booklets on Tudor and Stuart Civilization, 1963), he inveighed against this mistake, pointing out that the Armada battle was actually a draw, that Spain was a threat to England for years afterward, and that the "campaign of 1588 was not the end of Spain's Atlantic navy. It was its beginning." A careful reading of this cogent pamphlet, which is missing from the bibliography, might have improved some of the generalizations about Drake and sixteenth-century naval warfare.

But one should not quibble about details when such a handsome and useful book reveals so much valuable material. If the authors have made of Drake a greater man than he probably was, it is no more than most of us would like to do. He was a daring hero and a man of courage and imagination. Any age could do with more men like him.

LOUIS B. WRIGHT

National Geographic Society

ROBERT LACEY. *Robert, Earl of Essex*. New York: Atheneum. 1971. Pp. xiii, 338. \$8.95.

This is one of those "popular" biographies whose readership is deemed to lie outside the ranks of professional historians. "With the general reader in mind I have kept footnotes and references to a minimum, but have provided a guide to sources," we learn in the preface (p. xii). The first part of this statement is accurate; the second is not, unless a list of a dozen abbreviations used in the few footnotes constitutes such a guide. It is a pity that the author chose

thus to pitch his work because there are the makings of a good book here. For one thing, the character of Essex comes through very clearly; he was a petulant, spoiled child, self-centered and arrogant, given to tempertantrums and fits of the sulks when crossed, and at the end completely out of touch with reality, perhaps owing, says the author to tertiary syphilis. Second, the incompetence and capriciousness of the aging Elizabeth is emphasized, and properly so: any ruler who would trust a man like Essex with even a modicum of power was clearly guilty of bad judgment. The queen had lived too long; even her subjects thought so, and they greeted the accession of James I with great enthusiasm. The extent of her enfeeblement is indicated by the fact that, unlike her other favorites, Essex had no abilities at all, even in the military sphere, as the author's discussion of the earl's various campaigns makes clear. After the Irish fiasco and Essex's gross violation of orders, Elizabeth finally withdrew her confidence and drove him to his ill-conceived rebellion, which, in the author's view, might have succeeded if Essex had marched on Whitehall instead of the City on that fatal February Sunday.

The author shows considerable skill in handling his two central characters and in delineating their relations with each other, although he is extremely repetitious. Otherwise the book is not very enlightening. The level of analysis is illustrated by the remark that "sixteenth-century England was an autocracy and the Tudors were despots" (p. 27), and the author's acquaintance with recent scholarship is exemplified by the view that the increase in the size of the House of Commons in the sixteenth century "showed the growing power of the merchant class" (p. 100). There is room for a book on Essex as a symbol of what was going wrong in the last wretched decade of the Elizabethan regime. Unfortunately this biography is not it.

MAURICE LEE, JR.
Rutgers University,
New Brunswick

ANNE PALLISTER. *Magna Carta: The Heritage of Liberty*. New York: Oxford University Press. 1971. Pp. 133. \$6.50.

To trace the myth of Magna Carta from 1603 to 1970 led Anne Pallister through "a wide va-

riety of seemingly unrelated material" and into fascinating digressions on British law and politics. As the Charter's "continual interpretation and development" enriched the myth, Magna Carta's substance evanesced. The Petition of Right, after 1628, began to supersede the Charter as an emotive symbol of individual liberty, and after 1689 the Declaration of Rights was called "a new Magna Carta." Especially exciting are the big ideas that this little book expounds: the concept of "the constitution," a "doctrine of parliamentary sovereignty," and the "Repeal of the Charter." By 1970, only four chapters of the thirty-seven in the 1225 Charter survived, and three of these are simply pious sentiments—that the church, all freemen, and the City of London are to have their liberties. The fourth (ch. 29 [39]) still upholds the rule of law, due process, and trial by one's equals. Pallister's chapter on "The Charter Repealed" is novel and most illuminating: "almost half" the chapters were repealed in 1863, five more between 1879 and 1892, and acts of Parliament made the final decimations in 1925, 1947, 1948, and 1969. Attacks on Magna Carta are traced back through Tom Paine's skepticism in 1791 to John Wilkes's boredom in 1775 when he had become "sick of his part," Walpole wrote, as "protector" of the Charter. By then the historical school and evolutionists, like Burke, were rejecting "the ideas of immemorial liberties, the Saxon constitution, and other mythical notions" in favor of "overriding causal laws" and "a long gradual process [that] spread over generations."

The "constitution" and "parliamentary sovereignty" became doctrines to rival the myth of Magna Carta and in due course to supplant it as a political shibboleth. In 1736, the year after Bolingbroke had disclosed "the spirit of the constitution," the Whig *Daily Gazetteer* asserted that "the modern constitution is infinitely preferable to the ancient." Men had begun to take a prospective, rather than retrospective, view toward government and to glimpse the dawning cult of progress. Likewise, the idea of parliamentary sovereignty raised doubts about any fundamental law, even that in Magna Carta, and by the 1750s the very words, Magna Carta, had acquired an insubstantial quality, if not a hollow ring. However, the myth lingered on and became a cliché used

by radical-liberals, like Granville Sharp and Francis Burdett, in their political melodramatics. Pallister doubts "whether half of those who referred to it [Magna Carta] had any personal knowledge of its contents; they merely read back into it whatever political maxim or precedent they required." Time and again, she shows, the Charter "lost its relevance," and yet the myth has continued. However, she might have rejected another myth, that "in the fifteenth and sixteenth centuries the Charter declined in importance, but was revived . . . in the early seventeenth," had she consulted S. E. Thorne *et al.*, *The Great Charter* ([1965], 28-34). Actually, Henry VIII's lively politics had prompted Stephen Gardiner to tell how at Wolsey's fall "Magna Carta was spoken of," and Thomas More too cited it in his own defense as a higher law, if not a constitution. Then in the 1580s Magna Carta appeared as a superstatute in parliamentary debates and legal treatises like those of Lambarde, Beale, and Cosin. Parliament may yet deprive the four surviving chapters of the validity of law, but in popular politics, Magna Carta's myth, as a symbol of undefined rights and liberties, is likely to go marching on.

WILLIAM HUSE DUNHAM, JR.
Yale University

ALLEN G. DEBUS. *Science and Education in the Seventeenth Century: The Webster-Ward Debate*. (History of Science Library: Primary Sources.) New York: American Elsevier, 1970. Pp. 307. \$15.00.

The so-called Webster-Ward debate is well known to historians of seventeenth-century England, but in the absence of the texts students, as the editor here says, have had no easy time in deciding what the issues really were. Here are the sources: a short polemic by John Webster, Nonconformist minister and alchemist; a witty and trenchant reply by the Savilian professor of astronomy, Seth Ward; and a defense of universities as trainers of ministers by a Presbyterian minister, Thomas Hall.

The novelty in these works lies in the contrast between Webster's understanding of Bacon's plea for a "new learning" and that of Ward. Webster had a very feeble understanding of contemporary science, an imperfect comprehension of what Bacon, Descartes, and Gal-

ileo meant, and a profound respect for alchemy, natural magic, Robert Fludd, Rosicrucianism, and astrology. Seth Ward was a trained university graduate and an able mathematical astronomer, and he had a thorough understanding of the emergent new science. Webster's plan for reform of the universities, with which he probably had no firsthand acquaintance, consisted in a plea for natural magic and spagyric chemistry to replace the conventional curriculum. Dr. Debus rightly links Webster's mystic tendencies with the Comenian spirit and such Puritan reformers as Samuel Hartlib.

Pace Dr. Debus, I cannot see that Webster emerges as important; he is a minor and not very worthy exponent of Renaissance hermeticism, a wonderfully muddled and half-baked thinker who understood imperfectly the work of those whose names he drops so glibly. Ward rightly walks rings around him, amusingly demonstrating Webster's ignorance of universities, his misunderstandings of Bacon and of the new learning, and the genuine fallacies inherent in the appeal to natural magic. Ward is great fun to read and very informative; his essay helps to explain how Oxford became a major center for the new science in the 1650s.

MARIE BOAS HALL
Imperial College

MARCIA R. POINTON. *Milton & English Art*. [Toronto:] University of Toronto Press, 1970. Pp. xliii, 276. \$14.50.

This comprehensive and fully illustrated survey of a topic hitherto only briefly handled by a few authorities on Milton should significantly illuminate for cultural and art historians the fluctuations of taste through two centuries and for Miltonists the associated fluctuations in interpretations of Milton's poetry whose shallow and crosscurrented backwash still troubles our reading. The insight is chiefly provided by its 218 carefully cataloged monochrome reproductions, mostly good, chiefly from illustrated editions but also from sketches, exhibited paintings, and palatial garden-house decorations, from Medina in Tanson's 1688 *Paradise Lost* to the 1888 pastorals of Palmer from Milton's early poems. The illustrations are supported by brief but useful and fairly well-documented accounts of the il-

lustrators—their backgrounds, associations, and developing tastes, preoccupations, and techniques—and by descriptive commentaries whose framework is provided by the accepted commonplaces of art history about such matters as Baroque and Rococo, Neoclassic and Romantic, the terrible sublime and the curvaceously beautiful, and so forth. These are the principles that governed the older and now outmoded accounts of Milton's reputation and influence on which this survey seems principally to depend.

Some use is made of a few of the more recent comments on illustrations—Collins-Baker, Gardner, Svendsen, Hughes, though the full implications of Hughes's now twice-reprinted criticism of seriously misinterpretative illustrations of the Expulsion from Paradise seem to have escaped it, and Hughes unhappily finds no place in its index. But the author appears to be totally unaware of the efforts of so much recent Miltonic scholarship and criticism—symptomatic not only of further fluctuations in taste but of much more significant revivals—to resolve interpretive problems inherited from and to reassert essentially Miltonic principles obscured by such inadequate responses as are chiefly demonstrated by these two centuries of illustrations.

The survey more than achieves, though it does not always strictly limit itself to, its modest aim: "to recognize rather than evaluate." But it demonstrates not so much "Milton's influence over painting and graphic art" as the varied failures of response and taste in the artists. Save for the mature Blake (whose sensitivity as to the Son and the situations of the human pair are thrown into relief by what surrounds him as well as by his original handling of fashions of line and composition) and save for infrequent moments of apparently accidental sensitivity in a few other illustrators (as in the troubled Adam, tempted and detected, by Barry), it is difficult to escape the impression that the illustrators, supported by the esthetic theorists of the sublime and the beautiful and so forth, were chiefly engaged in further distorting those tastes—for the Baroque, the sublime, the sensuously naturalistic, and so on—which recent literary critics have been teaching us to see Milton manipulating for his superior purposes. It becomes clear from the survey

that, apart from Blake, there is not one adequately Miltonic "Expulsion" in the lot—a truly terrifying conclusion. And the process reaches a climax in Martin's lush but gloomily threatening Paradise landscapes, here greatly admired as highly "Miltonic," though it is noted that Martin (on account of his ineptitude in figure drawing) reduces the size and significance of Adam and Eve. He has in fact reduced them to pitiful victims of a God and a Nature whose gloomy ways are quite un-Miltonic and quite unjustifiable. It is a relief to turn thence to Etty's pneumatic Sabrina or Palmer's pastoral insipidities—though these hardly illuminate, save by their inadequacy, whatever Milton's masque is saying about chaste beauty or his *L'Allegro* and *Il Penseroso* about sensitively intelligent discrimination. Yet such lavishly illustrated inadequacies should make this volume of most stimulating interest to good refiners among students of Milton.

ARTHUR E. BARKER

University of Western Ontario

P. D. G. THOMAS. *The House of Commons in the Eighteenth Century*. New York: Oxford University Press. 1971. Pp. ix, 382. \$11.25.

ROMNEY SEDGWICK. *The House of Commons 1715-1754*. Volume 1, *Introductory Survey, Appendices, Constituencies, Members A-D*; Volume 2, *Members E-Y*. (The History of Parliament.) New York: Oxford University Press for the History of Parliament Trust. 1970. Pp. xv, 633; ix, 571. \$70.00 the set.

Dr. Thomas's book deserves a warm welcome. He fulfills admirably his aim to set out the main categories of business the House of Commons conducted, the way it was arranged and debated, the work of committees, the attendance and seating of members, and indeed every aspect of the practice and procedure of the Commons in the century in which it came into its own as an essential part of the government of the country. The central importance of the individual member is repeatedly emphasized, and the connection between the presence of so many independents, the way business was conducted, and the importance of debates is usefully drawn. The book has two principal strengths. The first is the mass of detail on all aspects of the work and procedure of the House, drawn, despite Dr. Thomas's modest

preface, from an impressive range of sources—all of it set out clearly and illuminated by informed and perceptive comments. The second is that it is not a static picture: we are constantly made aware of change and development—in the role and position of the Speaker, for example, or in the gradually increasing press of business that was eventually to restrict severely the time available to the individual member. A book on the procedure and the business of the House of Commons could easily be dull and lifeless; it is not the least of the virtues of Dr. Thomas's book that it is not only informative but also always lively and interesting.

The two handsomely produced volumes of the newest addition to the History of Parliament require a more qualified welcome. They are constructed on the same lines as their predecessors, edited by Namier and Brooke, on the last half of the century: an introductory survey by the editor; various appendixes setting out the number of contested elections, the sources of debates, and so on; a survey of constituencies and electoral contests; and finally—occupying the last third of volume 1 and the whole of volume 2—parliamentary biographies of all the members. There is much of value here. It is useful to have accurate information conveniently at hand about the parentage, education, and social and economic standing of every member of parliament between 1715 and 1754, as well as their recorded votes and other evidence of political activity. One can only be grateful for the labor that went into it. But many of the biographies are very slight indeed, because most members of the eighteenth-century House of Commons never spoke in debates. Not all are as snappy as that of the ill-fated Admiral Byng ("Returned on the Admiralty interest for Rochester, Byng was executed in Portsmouth harbour, 14 Mar. 1757") but a very large number inevitably contain little more than a few bare biographical details. Even the major biographies are shorter than in the Namier and Brooke volumes; Walpole, Pelham, and Pulteney are given much less than half the space that was made available for the biographies of Burke, Fox, and George Grenville.

That Walpole's biography is as much concerned with his skill as a courtier as with his

activity in the House of Commons is perhaps understandable in view of the role he came to play as a link between court and Parliament—a role about which Mr. Sedgwick writes interestingly though briefly in the introduction. But we learn very little in either the biography or in the introduction about how Walpole constructed and maintained his majority in Parliament. Indeed, this general theme of government management, so central one would have thought to the parliamentary history of this period, is disappointingly slighted in the volume. This derives perhaps from Mr. Sedgwick's view that the politics of the period are best explained in terms of the conflict of Whig and Tory parties. We are presented in the introductory survey with a continuation to mid-century of the party conflict of Anne's reign, long after the immediate problems and concerns that underlay that conflict had been solved or laid aside. This goes against the grain of all recent work on this period, and it needs to be argued and documented more fully than it is here. This is especially true of Mr. Sedgwick's treatment of the Tories, who are not only characterized as a united party, but more startlingly as a party devoted to the restoration of the Stuarts. At crucial points, in 1730 and 1741 for example, the behavior of Tory country gentlemen in the House of Commons is explained as a response to instructions sent from the Pretender. There is no doubt that Jacobitism was a more potent force and was more genuinely feared than has often been thought. But even if prominent Tories corresponded with the Pretender or were named as sympathizers in Jacobite reports, one needs a lot more evidence than this to be persuaded that the great body of Tory country gentlemen were "a predominantly Jacobite party, engaged in attempts to restore the Stuarts by a rising with foreign assistance" (p. ix). One can only say that the evidence for this statement has not been produced. Altogether it seems to me that Mr. Sedgwick's introduction presents an idiosyncratic view of the period.

J. M. BEATTIE

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BERNARD SEMMEL. *The Rise of Free Trade Imperialism: Classical Political Economy, the Empire of Free Trade and Imperialism, 1750–1850.*

New York: Cambridge University Press. 1970. Pp. x, 250. \$11.50.

The study of the nineteenth-century British Empire has attracted a number of first-rate minds in the era of imperial erosion since World War II. Older theories about the role of personalities, groups, and periodization have all been attacked, denounced, and discarded as "myths." It is by now well established that there was little distinction between Britain's policies during the so-called Little England era and those responsible for imperial expansion in the age of imperialism of the last quarter of the nineteenth century. John Gallagher and Ronald Robinson led the demolition squad that counted among its members Professor Bernard Semmel, who pointed out that the Philosophical Radicals accused by others for their Little Englandism were, in fact, proponents of a larger empire. Surveying the scene in the mid-sixties, Professor John S. Galbraith, whose own contribution to the explosion of the myths was no little one, despaired: "The ancient landmarks are in ruins and no new structure has been established to replace them." He noted as an exception the works of Gallagher and Robinson.

Professor Semmel's book marks an extension backward of the Gallagher-Robinson thesis. The latter had indicated that there was imperial extension all through the nineteenth century and that it was caused neither by Hobson's ganglion of financiers nor by Lenin's agents of inevitable monopoly capitalism but by British statesmen, who, irrespective of their partisan labels, supported the national interest of trade. Hence the free-trade imperialism of the nineteenth century. Professor Semmel provides a predominantly economic-oriented theoretical cushioning to the Robinson-Gallagher argument, which was derived primarily from official archives and private papers of prominent politicians. In this effort, Semmel stands out as a brilliant economic historian with an incisive mind capable of offering refreshingly new insights into the writings of classical economists. Mercantilism was dead, but pursuit of mercantilist interest persisted in the free-trade era to the point that imperial expansion was inevitable. Semmel achieves less than spectacular success, however, in showing the direct line between the economic theorists and colonial policy-makers, except in the one significant case

of a nonofficial, Edward Gibbon Wakefield.

Wakefield has been given more credit in this volume than that egotist would have allowed himself. Disagreeing with J. B. Say's law, Wakefield perceived a clear need for overseas markets for England's surplus manufactures. Hence his advocacy of systematic colonization involving territorial expansion. Did Hobson get his ideas from Wakefield? Yes, says Professor Semmel. Hobson read an English translation of the work of an Italian economist, Achille Loria, and Loria was a disciple of Wakefield, not of Karl Marx, as is widely believed (pp. 212-13). Why did not Hobson, who was anticipated by Wakefield, read the latter's works in the original English? There are many such loose links in a long list of anticipations to be found in the volume (pp. 77, 89, 175, 215, 222-23). I would modestly point out that Professor Semmel has been partly anticipated by Donald Winch, *Classical Political Economy and Colonies* (London, 1965), and R. N. Ghosh, *Classical Macroeconomics and the Case for Colonies* (Calcutta, 1967). All said, however, Professor Semmel has produced a book that should prove stimulating to students of imperial history.

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BRIAN INGLIS. *Men of Conscience*. New York: Macmillan Company. 1971. Pp. 437. \$10.00.

Dr. Inglis has produced a thoroughly pessimistic view of the condition of the poor in England during the Industrial Revolution. His target is "political economy," a complex of ideas that conformed marvelously to the economic interest of men of property and justified what he considers to have been their callous disregard for the welfare of laboring people both in country and town. Gripped by the notions that a free economy was self-regulating, that private property was sacred, and that Malthus was right about population, the rulers of England abdicated responsibility for the welfare of the poor. Fighting gallantly but hopelessly against the victory of laissez faire were a few "men of conscience"—people as various as Cobbett, Paine, Southey, and Robert Owen. Dr. Inglis believes mightily in villains and heroes.

One suspects that enthusiasm for his cause may sometimes threaten Dr. Inglis's historical objectivity. The evidence heard by Sadler's Committee on Factory Children's Labour is perhaps not quite as dependable as Dr. Inglis suggests. He condemns the report of the Poor Law Commission of 1834 not merely for inadequacy or blindness or wrongheadedness but also for moral delinquency. The commissioners, he writes, "manipulated the evidence"; their "proposals . . . were not based on the evidence: the evidence was selected, and presented, to justify the proposals." This is strong stuff, and it is not supported, for example, by J. D. Marshall, who observes in his *The Old Poor Law, 1795-1834* (1968) that "one does not need to disparage the integrity of purpose of the Commissioners to perceive that they produced some very misleading data." Similarly, Dr. Inglis insists on the declining standard of living of farm laborers during the first third of the nineteenth century, quoting contemporary opinion and Lord Ernle in support of his view. He dismisses, without examination, the opinion that "after 1820, at least, living standards began to rise." Yet Chambers and Mingay in their standard work *The Agricultural Revolution, 1750-1880* (1966), which he does not list in his bibliography, state precisely that "after the postwar fall in wages had ended in 1824 . . . there can be no doubt that the majority of labourers were better off than they had been in 1790."

Men of Conscience is a large, well-written tract, full of lore about the masses who suffered and the classes who oppressed them. It is very far from judicious, impartial history.

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JOHN BUTT, editor. *Robert Owen: Aspects of His Life and Work. A Symposium*. New York: Humanities Press. 1971. Pp. 265. \$11.50.

This book is a collection of essays written for the most part by the able young historians of Strathclyde University. The editor explains, "As New Lanark is virtually on our doorstep, we felt that we should not allow the bicentenary of Robert Owen's birth . . . to pass without a collective effort to put on record our views on aspects of his life and work" (p. 9). "Biography is essentially our purpose," Dr. Butt writes. "We have concentrated on Robert Owen and

not on Owenism" (p. 16). But the book reflects its origin rather than its aim. Two of the seven essays have little to do with Owen. One relates the story of Orbiston, a Scottish Owenite community that existed and died while Owen was in the United States overseeing the affairs of New Harmony. Another essay is entitled "The Industrial Archaeology of New Lanark"; it concentrates on the history of the buildings and emphasizes the years of greatest construction and renovation, years when Owen was not associated with the business. The material is interesting, but it has no place in a book whose purpose is "essentially" biography.

The biographical sections of the book treat Owen thematically: his social and economic thought, his role as an educator, his relations with workingmen and their movements, his role in the agitations for factory reform, and his career as a businessman. The contributors obviously worked with each other, and there is very little repetition. The essays are broad in scope and cover his career beyond New Lanark; they are good summaries, explaining the basic direction of his approach to all these areas. Dr. Butt's own contribution on Owen as a businessman is outstanding. He offers us new information on Owen's precarious personal financial position before 1814, on the tangled relations of the 1810-14 New Lanark partnership, and on the customers and profits of the business. The other essays cover ground that has been well explored previously, and consequently they contribute less to Owen scholarship. Their total effect is to give only a glimpse of his complexity. There is simply too much to Owen for five good essays to cover adequately.

The problem with the book is that it has no clearly formulated historical purpose. The stated purpose of biography sits badly with the table of contents. The biographical articles land awkwardly between broad coverage and contribution to scholarship. The articles are all good, but they do not add up to anything substantial.

THOMAS MILTON KEMNITZ
University of New Hampshire

F. L. WISWALL, JR. *The Development of Admiralty Jurisdiction and Practice since 1800: An English Study with American Comparisons*. New York: Cambridge University Press. 1970. Pp. xxvii, 223. \$13.00.

This volume, a doctoral dissertation in law, won the Yorke Prize of the University of Cambridge in 1968. It is not a history of the admiralty court but rather a study of the development of admiralty jurisdiction and practice in England in the nineteenth century because the "period covered has seen the development of most of the jurisdiction and substantive and procedural Law of Admiralty which prevails in the present day" in both England and the United States.

Mr. Wiswall begins his study with a description of the procedure of the admiralty court when it "awakened from a slumber of centuries" in the time of Lord Stowell, famous in international law for his Prize Court decisions; these decisions, however, are not dealt with in this book, which is deliberately restricted to the instance and civil jurisdiction of the admiralty court. Initially he relies on two famous textbooks of admiralty law and practice, Browne and Marriott, both published in 1802 (equally influential in the United States). He traces the growth and expansion of admiralty jurisdiction through Lord Stowell's decisions in the admiralty court, and clearly brings out the reciprocal impact and influence of English and American admiralty law in his discussion of the decisions of the great admiralty judges, Lord Stowell and his contemporary, Mr. Justice Story, on the other side of the Atlantic, cited equally as authorities in the courts of both countries.

The history of the court is traced through its second great period, the judgeship of Dr. Stephen Lushington, and the end of the control of the court by men carefully trained in civil law, as distinct from common law. The Court of Probate Act of 1857, which also permitted the dissolution of the College of Advocates more popularly known as Doctors' Commons, a body for the civilians somewhat analogous to the Inns of Court for the common lawyers, ended the monopoly of specially trained civil lawyers. The impact of the admission of lawyers trained only in common law was compounded by the reorganization of the judiciary in the Acts of 1873, 1874, and 1875. A concluding chapter supplies a highly technical analysis of this impact in terms of the evolution and in some cases misinterpretation of the action *in rem* through the decisions of the judges now trained for the most part in the common law.

Mr. Wiswall is not a historian but "a mariner and lawyer." Fascinated by the interaction of the English and American admiralty courts, and eminently qualified because of his training in both American and English admiralty law, he has written a thoughtful and valuable monograph within the limits he has set.

MADELINE R. ROBINTON
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A. ASPINALL, editor. *The Correspondence of George, Prince of Wales 1770-1812*. Volume 7, 1810-1811; Volume 8, 1811-1812. New York: Oxford University Press. 1970; 1971. Pp. 444; 577. \$27.00; \$33.75.

The present series of the Prince of Wales's private letters ends in February 1812, where it joins chronologically to the three volumes, which appeared back in 1938, of his correspondence during the periods of his unrestricted regency and kingship as George IV. The last volume of this second series provides a comprehensive index to the series as a whole, as well as a hundred pages of miscellaneous letters with dates scattered over his entire career that for one reason or another had been previously omitted.

The major political development during the years covered by the seventh and eighth volumes is the two-stage establishment of the regency following the mental breakdown of George III. Some letters describe the singular condition of the mad monarch; others provide insights into the political maneuverings that his condition inspired. Perceval emerges as a strong and practical Tory leader; Lords Grey and Grenville as hesitating and uncertain; the prince as a loyal son who quiets both his physical pains and his fears with liberal doses of laudanum. The gradual transformation of the Whig George into the Tory George is perhaps the most important political theme in the events described.

Many of the letters deal with the affairs of the royal family. The financial arrangements for the permanent regency are discussed almost *ad nauseam*. An appendix to volume 7 provides some two dozen depositions relating to the attempt by the valet Sellis to assassinate the duke of Cumberland, and includes the curious confession of Charles Jones, who alleged the

roles were actually reversed. There is more than a little human interest in the disapproving memorandum by Lord Eldon regarding George IV's will, and in the last two letters penned by Mrs. Maria Anne Fitzherbert to George.

Students of British government may find items of interest in the patronage letters that stud the correspondence, and those specializing in medical history should revel intellectually in the numerous illnesses and remedies described throughout. Social history may be enriched somewhat by an excellent cache of letters dealing with the history and management of the London theaters.

Historians concerned with international affairs may be a trifle disappointed at the paucity of such documents in the collection. Lord Wellesley, the foreign secretary, only rarely expressed his personal thoughts and reactions to the important information he transmitted to the regent. There are a dozen or so letters that should interest Latin American specialists, a handful that mention Anglo-American affairs *en passant*, and an informative memorandum on the state of Prussia in 1811, but scholars seeking facts regarding the deterioration of Anglo-American or Franco-Russian relations during this period will come away virtually empty handed.

The high standards of professional editing set by the previous volumes are maintained throughout. One can hardly conclude a review of these final volumes without noting the splendid contributions to historical studies of Arthur Aspinall in these and all of his former publications.

WILBUR DEVEREUX JONES
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J. K. JOHNSON, editor. *The Letters of Sir John A. Macdonald, 1836-1857*. (The Papers of the Prime Ministers, Volume 1.) Ottawa: Public Archives of Canada, 1968. Pp. xxiii, 600. \$10.00.

J. K. JOHNSON and CAROLE B. STELMACK, editors. *The Letters of Sir John A. Macdonald, 1858-1861*. (The Papers of the Prime Ministers, Volume 2.) Ottawa: Public Archives of Canada, 1969. Pp. xvii, 626. \$10.00.

These volumes of the letters of Canada's first prime minister, Sir John A. Macdonald, represent the beginning of an ambitious Canadian Archives project. The series, which revives an

Archives publication program suspended some years ago, is intended to make available to scholars the significant writings of the Canadian prime ministers since Confederation. The Public Archives of Canada is in a much more favorable position to undertake such a task than are similar institutions in the United States or Great Britain. The papers of United States presidents are scattered from Hyde Park to Texas, and the British National Register of Archives must keep track of prime ministerial collections in a variety of depositories. The principal papers of all of Canada's prime ministers through John Diefenbaker and Lester Pearson are housed in the Public Archives building in Ottawa.

While a considerable number of the Macdonald letters in the series' first two volumes have been printed before, many of them have been known only to specialists. Every effort has been made to locate and index or publish scattered Macdonald letters from private and public sources other than the Archives' own massive collections. In addition, careful editing provides the reader with the kind of collateral information about people, events, and places on which scholars must otherwise expend a great deal of time and effort. A comprehensive calendar of letters for each volume goes beyond the varied selections printed in full. The series has other useful appendixes as well as some illustrations.

Readers of the Macdonald letters who are familiar with the details of Canada's history will find considerable interest in tracing the intellectual development of the young Upper Canadian lawyer and budding politician, who was later to be considered the chief architect of Confederation, a leading advocate of harmony between the French and the English, a staunch defender of the imperial tie, and a rather anti-American Canadian nationalist as well. Some of these judgments are borne out by the early letters; others reflect changes that were clearly the result of time and a great deal of political experience. The friend and ally of Sir Georges Cartier could write in 1847, "The chief cause of the unanimity of the U. C. Conservatives in supporting the present Ministry is a desire to sustain His Excellency in his residence to the factious demands of the French Canadian party" (1:55). Or, ten years later,

Macdonald could make the revealing comment to his sister Louisa, "While Mr. Papin is making a French speech, I snatch a moment to write you" (1:425). The young Macdonald placed himself firmly in opposition to Canada's leaving the Empire in his 1849 comments on the British American League, which he thought would "put its foot on the idea of annexation" (1:157). By 1853 he was skeptical of Britain's support in times of crisis. "As things are looking warlike in Europe, it is probable John Bull will be withdrawing his troops from Canada and leaving us to shift for ourselves" (1:197). As the attorney general for Upper Canada, Macdonald became thoroughly involved in the military response to the *Trent* affair of 1861 and what he called "the unsettled state of the relations between Great Britain and the United States of America" (2:418). He had earlier in that year assured the railway promoter John A. Poor of Portland, "I fully appreciate the paramount importance of the Reciprocity Treaty and shall spare no pains to keep it before the public here" (2:285).

The first two volumes of the series carry the writings of Canada's first prime minister to 1860; students of Canadian, North American, and British history in the nineteenth century can only look forward to forthcoming publications in this admirable project.

ALICE R. STEWART
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Orono

ROBERT BLAKE. *The Conservative Party from Peel to Churchill*. Based on the Ford Lectures delivered before the University of Oxford in the Hilary Term of 1968. New York: St. Martin's Press. 1970. Pp. xii, 305. \$12.50.

In assessing this book it is important to remember that it was first conceived as a series of public lectures, that it is directed at a wide audience, and that it covers a century and a quarter of political history in a severely limited space. The professional historian runs the danger of skating on thin ice when he embarks on a broad interpretive study that cannot rest solidly on a complete foundation of his own research and that is aimed at more than an academic audience. Nevertheless, it is important that good historians be prepared to take these risks and not leave such necessary and impor-

tant work to amateurs or charlatans. Robert Blake has undertaken such a task and carried it out successfully. It is to be hoped that the book will go into paperback to gain the wider audience that it deserves.

The extent of the book's success, of course, is limited by the amount of research that has been completed and published on the party politics of the whole period under review. Consequently it cannot be definitive as were Blake's earlier studies of Bonar Law and Disraeli, based on his own thorough research. The chapter on Lord Derby, whose biography Mr. Blake has undertaken to write, has many interesting quotations from the Derby papers, while that on Disraeli makes much use of a variety of recent monographs, as well as of his own great knowledge of the subject. The chapter on Lord Salisbury is perforce much thinner for lack of such a base. He draws an excellent brief pen picture of that statesman, whom he describes as "the aloof, sceptical inheritor of a great name and a famous house, standing rock-like in the advancing tide of democracy, emblem of a vanishing world and, although a great Foreign Minister, essentially negative, indeed reactionary, in home affairs"; but half the chapter deals with Conservative party organization going back to the Peel period. The Salisbury ministries receive only seven pages and the Reform Act of 1884, which Salisbury resisted so fiercely, only two passing allusions, although ten pages are devoted in the previous chapter to the Reform Act of 1867. Much work remains to be done on the political history of the eighties and nineties, and we may regret that Mr. Blake has not chosen to write the life of Lord Salisbury, which he could do so well.

He has little to say about the reactionary aspect of conservatism in the period 1905-14, but pays much attention to what he regards as the baffling aberration of the tariff reform movement in the party in those years. Baldwin is treated sympathetically as a friendly and considerate man. "Yet in the end," Blake comments, "Baldwin failed because the wounds on English society were not the result of malice, cruelty or spite, and could not be cured by kindness." The assessment of Churchill is especially good, although some will think that the author is too kind to the second ministry. Blake emphasizes Churchill's mistakes in the interwar period and

his unpopularity in the party, even when he became prime minister, but notes his extraordinary political success in the war years. Although Churchill was punctilious in observing traditional forms, "the fact remains," Blake observes, "he ruled England for five years with a degree of authority such as no Prime Minister has ever possessed before or since."

In his preface the author says that he is not writing a history of the Conservative party but rather a commentary on it. Nevertheless, the book follows a chronological pattern and contains a good deal of factual material necessary to inform the general reader if the commentary is to be meaningful. The main events in the party's history in the years 1830-1955 are touched on, some in fair detail; the results of every election are studied methodically, and party organization is dutifully examined in its various transformations. But Mr. Blake is at his best with personalities, as is to be expected from the brilliant biographer of Disraeli. His sense of history and flare for its dramatic presentation are frequently seen in his love of drawing historical parallels, some obvious, some more obscure. He sees similarities in the position of the Conservatives, 1846-66, and that of Labour, 1918-40, in the roles of Bentinck and Lansbury, in the effect of the Jameson Raid of 1896 and the Suez raid of 1956. He also makes intriguing, if less obvious comparisons between Disraeli's situation in 1867 and Eden's in 1956, and between the inevitability of the Labour victory in 1945 and the Aberfan disaster of 1966.

In his conclusion Mr. Blake has some interesting things to say about the continuing characteristics of British conservatism. Earlier he recognizes a real break in Conservative history in 1846, but he finds many of the characteristics of Peel's Conservatism, which he admires, recurring up to the present day. He rejects the role of the stern unbending Tory on the one hand and of the radical Conservative innovator on the other. "The Conservatives can accept change," he writes. "They can even initiate it, once they are safely in office. . . . But experience suggests that they should be very chary about announcing major changes as the theme of an election programme. If the country is in the mood for radical change it will not vote Conservative." The book is full of shrewd com-

ments, some amusing, some controversial, some penetrating. In the final paragraph, however, the author reveals his real reason for writing it, and it is one with which I cannot quarrel. "History is not an exact science," he writes, "and it never will be. It is a good story. The fortunes of a political party, like those of an individual, make a fascinating tale, and that, rather than general truths or lessons from the past, must be the justification for this survey."

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L. PERRY CURTIS, JR. *Apes and Angels: The Irishman in Victorian Caricature*. Washington: Smithsonian Institution Press; distrib. by George Braziller, New York. 1971. Pp. xi, 126. \$6.95.

In *Apes and Angels*, Professor Curtis explores further the theme of anti-Irish prejudice in Victorian England he first developed in his valuable psychological study, *Anglo-Saxons and Celts* (1968). What he shows here, with a wealth of illustration from Victorian comic artists like Tenniel and Leech, is "the gradual but unmistakable transformation of Paddy . . . from a drunken and relatively harmless peasant into a dangerous ape man."

Gillray had portrayed the Irish peasants of 1798 as brutal and degraded; *Punch* in 1843 went further in representing Daniel O'Connell as having conjured up "the Irish Frankenstein." Not, however, until the fears aroused in England by the Fenians in the 1860s, did London caricaturists depict Irishmen as apes. (In 1860 Kingsley had referred to Connemara peasants as "white chimpanzees.")

Curtis makes the interesting suggestion that Victorian Englishmen, disturbed by Darwinian speculation that men and apes might have evolved from a common ancestor, may have found psychological relief by representing "The Irish and other lesser breeds . . . as a buffer or evolutionary cordon sanitaire between themselves and the anthropoid apes." A writer in *Punch* implied in 1862 that the Irish peasant ("The Irish Yahoo") might even turn out to be the "missing link" between man and gorilla (the first live gorilla had actually arrived at the London Zoo in 1860). For months after the Phoenix Park murders in 1882, writes Curtis, "London's comic weeklies were filled

with cartoons of ape-like monsters with huge mouths and sharp fangs."

Perhaps the chief implication of this study is that, underlying all the familiar rationalizations for English rule in Ireland was a deeply-held, irrational conviction that the Irish, being an inferior species, were by nature incapable of self-government and unfit for Home Rule. Since today, also, nations hold grossly distorted images of one another, a psychological investigation like this, marked by such knowledge and insight, has significance far beyond the field of Anglo-Irish relations. The value of this revealing study is greatly enhanced by the numerous well-reproduced illustrations from *Punch* and other magazines. There are also some extremely interesting photographs of Fenian prisoners from the Dublin Castle archives.

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W. S. HAMER. *The British Army: Civil-Military Relations, 1885-1905*. New York: Oxford University Press. 1970. Pp. xii, 293. \$10.50.

"We are a great Military Power when we consider the amount of charge we bear for military purposes," said Sir Charles Dilke in 1893, "and we are a small Military Power, indeed, when we consider the result in efficiency." To resolve that lack of efficiency, any number of debates took place in Britain during the late nineteenth century—in Parliament, in the War Office, the cabinet, and in the periodical and newspaper press. The extent of that debate and the reasons behind it are the subject of Mr. Hamer's book. He quotes the third marquess of Salisbury who summarized the issue in 1901 by saying that from military men "who know the language and spirit of the War Office, it is easy to detect a desire that military problems shall only be solved by military men; but any attempt to take the opinion of the expert above the opinion of the politician must, in view of all the circumstances of our Constitution, inevitably fail."

It was a crucial problem. In a time of territorial expansion the British never developed an adequate military system. Considering the military resources to support it, Liddell Hart once called the British Empire of this period a magnificent game of bluff. Mr. Hamer hardly

needs to apologize, as he does in his introduction, when he says that any study of warfare in the nineteenth century has little relevance for that of the present day. The point should really be its relevance for the historian of Victorian Britain. On this score the book has one great strength and a number of significant flaws.

First, the scholarship in contemporary sources is almost impeccable. From private papers that have been accessible for some time to those more recently opened, such as the papers of Arnold-Forster, Dilke, and St. John Brodric; from the large number of Parliamentary Papers to the monthly and quarterly periodicals, the author has done a systematic study for the twenty years forming the chronological bounds of his subject. The only serious gap in the period itself is the *Journal of the Royal United Services Institute*, a strange omission, given his preoccupation with what is called the "military" as distinct from the civil servants and the politicians.

This tendency to dwell on abstract terms constitutes one major flaw of the book. It arises in part from an exaggerated concern with the scholarly apparatus of the subject, in part from a preoccupation with the thesis. As a result, there is little of the real army. Reports from royal commissions are discussed at length, and repeated quotations are given from periodicals, but there is little curiosity about who the people were. If the Royal Commission on Warlike Stores in 1887 was so significant, as it undoubtedly was, why not dwell at least briefly on its chairman, who was no less a man than Fitzjames Stephen? Why ignore so completely Sir Redvers Buller, who was adjutant general through this period for longer than any other individual and who wrote extensively to Campbell-Bannerman? On the side of the "civilians," we are told a good deal about the role of the secretary of state for war, little about the three permanent undersecretaries who literally ran the War Office—men such as Sir Ralph Thompson, Sir Ralph Knox, and Sir Arthur Haliburton. They appear as nothing more than names on the printed page, whereas they might have been grouped in an examination of the abstraction, "civil servant."

Because of this fascination with sources and thesis the book does not develop. Discussion of

public opinion and the party system tends to be peripheral and repetitious, without the balance that might have come from including the work of recent scholars. Mr. Hamer writes as though he were the first to investigate this subject, ignoring the conclusions of Brian Tunstall in the *Cambridge History of the British Empire*, of Brian Bond, Joseph Lehmann, and Jay Louvaas. It is a pity. Lacking awareness of what some among his own contemporaries have been doing, Mr. Hamer makes obvious and confined judgments that are not equal to the sustained patience of his scholarship.

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MARVIN SWARTZ. *The Union of Democratic Control in British Politics during the First World War*. New York: Oxford University Press, 1971. Pp. xiv, 267. \$10.25.

In August 1914 prominent Liberals and a leading member of the Independent Labour party, Ramsay MacDonald, formed what was to become the largest organization of nonpacifist critics of British intervention in World War I. Calling for parliamentary control over foreign policy, for an end to secret diplomacy, for the creation of machinery for securing international agreement, and, especially, for a negotiated peace whose terms would neither humiliate Germany nor provoke her revenge, this organization—the Union of Democratic Control—remained active into the twenties. Its importance has been recognized by many. Arno J. Mayer in *The Political Origins of the New Diplomacy* (1959) and A. J. P. Taylor in *The Trouble Makers* (1958) have told us much about its policy and intellectual antecedents. R. E. Dowse, in *Left in the Centre* (1966), has described the close cooperation that developed between the UDC and the ILP and how most Liberals supporting the former decided to join the ILP because its foreign policy so nearly accorded with their own. Dowse's "The Entry of Liberals into the Labour Party, 1910–1920" (*Yorkshire Bulletin of Economic and Social Research*, 13 [1961]: 78–83), and Catherine Ann Cline in *Recruits to Labour* (1963), have shown how, through the ILP, these Liberals came to play an active role in the Labour party.

These two studies along with Henry R. Winkler in "The Emergence of a Labor Foreign Policy in Great Britain, 1918–1929" *Journal of Modern History*, 28 [1956]: 247–58) have also shown how greatly the Liberal recruits influenced Labour's foreign policy after the war.

Professor Swartz has synthesized these accounts, adding new detail from original sources and several observations of his own. His study emphasizes the extraordinary effectiveness of the UDC as a nonpartisan, issue-oriented protest group. It suggests the extent to which the ILP welcomed middle-class recruits during and after the war; and, while reminding us that the UDC participated not only in the rise of Labour but also the decline of the Liberals, it further demonstrates the marked reluctance on the part even of radical Liberals to sever old party ties. Some, like C. P. Scott, broke with the UDC in September 1914 when it began publicly to criticize the prewar diplomacy of Asquith and Grey. Others, including the Liberal founders of the Union—E. D. Morel, Arthur Ponsonby, C. P. Trevelyan, and Norman Angell—may have been willing to criticize the record of their leaders in office. But even they did not break with their party until Lloyd George had betrayed Liberal principles once and for all. The story of their hesitation and their reasoning, well documented by Swartz, makes interesting reading. More important, since it confirms the point made by Trevor Wilson in *The Downfall of the Liberal Party* (1966), namely that it was World War I that initiated the process of disintegration in the party, those of us who are inclined to trace this process back to an earlier date must now take account of such lingering feelings of loyalty as Swartz has described.

BARBARA MALAMENT
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CAMERON HAZLEHURST. *Politicians at War, July 1914 to May 1915: A Prologue to the Triumph of Lloyd George*. New York: Alfred A. Knopf, 1971. Pp. 346. \$8.95.

This thorough and well-argued study largely replaces the first volume of Lord Beaverbrook's series on British political intrigue during the First World War. Dr. Hazlehurst wants to produce two more volumes carrying the study up

to Lloyd George's advent to power in 1916, and it is to be hoped that they will be as stimulating as the first.

Dr. Hazlehurst does not shirk controversy. Almost on the first page, he remarks that, until 1915, "Lloyd George the intriguer, coalitionist, and compulsionist exists only in fable." This is precisely opposed to conventional opinion, or to put it more accurately, to received myth. But as he points out, there is little sound documentary evidence that Lloyd George was plotting in 1915 to depose Asquith, and there is some evidence that he hoped to strengthen Asquith's cabinet. Perhaps the fact that Lloyd George alienated two parties, the Liberals in 1916-18 and the Conservatives in 1922, explains why accusations linger against him. He left behind him no party whose historians were committed to his defense; Dr. Hazlehurst hopes to provide a more balanced view than the traditional Asquithian one.

Most of the book examines the political crisis of May 1915, in which the Liberal government headed by Asquith was replaced by the Liberal-Conservative coalition, also under Asquith. Dr. Hazlehurst attacks the theory that the crisis resulted from a plot by Balfour, Churchill, and Lloyd George. The conspiracy thesis has always been somewhat tenuous, and this book rejects it convincingly. Perhaps Dr. Hazlehurst is rather hard on Professor S. E. Koss, as the conspiracy thesis forms a relatively small part of his book on Haldane. However, it and related arguments stressing Lloyd George's scheming ambition are central to Professor Trevor Wilson's book on the Liberals, which Dr. Hazlehurst describes as "sparsely documented," "not persuasive," and characterized by "deductive gymnastics."

One criticism of this book is that although it discusses in some detail the role of the *Times* in the May crisis, it does not consider the press as a whole during 1914-15. However, this is a large topic that Dr. Hazlehurst may be saving for the second or third volume. Nor is there a bibliography, and despite Dr. Hazlehurst's view, many books on the period are little known even to scholars. However, these are but minor criticisms of a well-written and stimulating work.

MICHAEL KINNEAR
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LEON Ó BROIN. *Dublin Castle and the 1916 Rising*. New York: New York University Press. 1971. Pp. 192. \$6.95.

The title of the American edition of this well-regarded book is slightly misleading: it is more than an account of what happened in and to the Dublin General Post Office in April 1916. The central theme lies in the story of Sir Matthew Nathan, effective head of the Irish administration from 1914 until his retirement in semi-disgrace after the Rising. Nathan was sent to Dublin to pave the way for Home Rule, the Liberals' solution to Ireland's political problems. His failure is a matter of record, but the book convinces us that he did not deserve the appellation so quickly seized upon by his critics, "Nathan the Unwise."

Nathan's political effectiveness had been considerably undermined, before his arrival, by the government he served. The inexplicable weakness of the Liberals in dealing with the chaotic aftermath of the Home Rule act of 1912 had served to cast doubt on constitutional solutions, to revive old hatreds, and to discredit British promises. But the major threat to Castle policy remained unknown until the Rising—the small, supersecret cadre of dedicated revolutionists prepared for and committed to an uprising, whatever its cost. For much of his tenure in office, Nathan was plagued with rumors of rebellion, by demands for strong action against the "Sinn Feiners," and with the "seditious" activities of a considerable portion of the populace. But with a few minor exceptions, Nathan did nothing. The author pays attention to the pressing need for Irish recruits for the British army, showing that any official action that might have increased anti-British feeling and lessened the flood of recruits was to be avoided. But the recruiting problem provides only a partial answer to the question of why Nathan chose not to act. Perhaps that he was "an uncommonly good man" takes us somewhat closer to an explanation. How could a liberal-minded administrator, wedded to constitutionalism, cope with a deeply rooted conspiracy without violating his principles? Nathan's answer was to choose "minimum action and maximum inaction" as a guiding principle. It was not enough.

GALEN BROEKER
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AARON S. KLIEMAN. *Foundations of British Policy in the Arab World: The Cairo Conference of 1921*. Baltimore: Johns Hopkins Press. 1970. Pp. xiv, 322. \$10.00.

Like many historical works, the title of this one does not do it complete justice. The book is not merely a study of the Cairo Conference of March 12–24, 1921, but an attempt to use that conference as a “prism” (p. viii) through which to understand British Middle Eastern policy as a whole in the crucial years after World War I. Thanks both to the conference’s having been the high-point in the formulation of that policy and to the author’s competence as a historian, the attempt is a successful one.

Nearly half of the work is devoted to the events of the few years leading to the Cairo Conference—an introductory chapter on British “Wartime Commitments,” then four chapters on the years 1919–20, with due attention given to developments in the Fertile Crescent, in Paris, and in London. Only one chapter deals with the conference itself, while most of the second half of the book is given to examining the implementation of the decisions made at Cairo in Iraq, Palestine, and Transjordan. A thoughtful chapter on “Perspective and Conclusions” is followed by several valuable appendixes (primarily documents from Foreign Office files concerning Churchill’s exchanges between Palestinian Arab and Zionist representatives in 1921), an extensive bibliography, and a series of useful, if only partial, biographical sketches of many of the individuals mentioned in the text.

In regard to its main subject—the formulation of British policy in the Fertile Crescent from 1918 to 1921—the work is excellent. Based on newly opened British archival material, it provides a clear, comprehensive, and convincing description of the evolution of that policy. More than earlier works on the subject, the nature of the sources available to the author makes possible the presentation of the total context of British Middle Eastern policy, the rival demands of domestic, European, Indian, general imperial, and great-power considerations upon shaping a policy for the area. To mention but the most valuable points that I found in the narrative: the hydra-headed nature of British policy making in the Middle East (with the India Office, the Foreign Office,

old Arab Bureau hands, pro- and anti-Zionists, and a galaxy of would-be proconsuls of empire, each providing a different input) is abundantly documented here; the force of motives of economy in shaping British policy is brought home repeatedly; and the evolution of the most important Cairo policy, the “Sharifian solution,” which installed Faisal in Baghdad and Abdullah in Amman, is traced brilliantly from its tentative beginnings through its hesitant and nearly reversed implementation to its hardening into dogma (“What had begun as an exercise in pragmatism had been expanded at Cairo into a principle to be applied wherever possible, beginning with Mesopotamia and then spreading to Arabia and Transjordan” [p. 124]).

The book’s weaknesses lie at its temporal and topical extremities. The introductory chapter on the war years seems overassertive even for a summary, too straightforward to do justice to the incredible complexity of the maneuvers of those years (for example, McMahon was “authorized to make” a statement to Husayn, not “the following statement in his letter of 24 October” [p. 10]). Most curious was the decision to halt detailed examination of the implementation of the Cairo decisions at the end of 1921. In the case of each area, a more logical point would seem to be in mid- or late 1922 (Iraq, the Treaty of October 1922; Palestine, League approval of the mandate in July 1922; Transjordan, the Treaty of December 1922). Yet developments after December 1921 are treated only briefly at the end of the chapters on each of these areas. Finally, in a book about policy there is relatively little about the policy makers themselves; other than Churchill’s energy and Abdullah’s cupidity, we learn little about the personalities and motives of the participants, British or Arab, in the events covered in the narrative.

JAMES P. JANKOWSKI

University of Colorado

DAVID GOLDSWORTHY. *Colonial Issues in British Politics, 1945–1961: From ‘Colonial Development’ to ‘Wind of Change.’* New York: Oxford University Press. 1971. Pp. xi, 425. \$14.50.

In analyzing the interaction of colonial events with domestic politics in postwar Great Britain, David Goldsworthy’s choice of topics will

be a matter of regret to some students of the period. While his selective, topical approach is defensible and often revealing, Goldsworthy eschews discussion of such central problems as India and the Middle East, maintaining that such "top-level policy issues . . . were not to be confused with the unspectacular activities of the Colonial Office." Nor does he treat in any detail South Africa's hardening policy of apartheid; regrettably so, since events there heightened conflicts in British settler colonies elsewhere on that continent. Yet all scholars in the field are in Goldsworthy's debt for his far-ranging explication of those topics that he takes under close scrutiny. Commendably, he sets his stage beyond the confines of the Colonial Office, wider even than a government-centered account of colonial devolution. His concern with "followers as well as leaders, out of office as well as in," enables him to deal effectively with that most elusive aspect of the political process, "influence," whether of the pressures of opposition upon government, backbenchers upon party leaders, or organized pressure groups upon political parties.

The accomplishments of such groups working with, though not always within, the Labour party is evidence of Labour's greater interest in the process and results of decolonization. Though the actual pace of change in Africa and elsewhere outstripped Labour's gradualist goal of "guiding" the colonies to responsible self-government, the party deservedly retained its self-image as a nonimperialist party supporting the cause of indigenous colonials. Through its opposition to the 1953 imposition of the Central African Federation upon unwilling native peoples, Labour sharply demarcated its colonial attitudes from those of the governing Conservatives; the bipartisanship that had once characterized British colonial policy lapsed for the balance of the decade. The Conservatives, "less interested in colonies as such than in the British Empire as a whole," responded to colonial pressures with less foresight, if more passion. The successive stages of British withdrawal—the abandonment of imperial preference, the evacuation of the Canal Zone, the surrender of the "fortress colony" of Cyprus—were masked by imperial rhetoric. By 1960 the Tories had surprisingly little stake left in the colonies, save the cause of the Euro-

pean settlers, which could not, after all, be sustained, their leaders discovered, in face of "the wind of change." In yielding to native demands, the Conservatives embarked upon a course again shared with Labour. In his meticulously researched book, Goldsworthy has successfully clarified the various pressures that led Labour, and drove the Tories, along the path of decolonization.

JOHN F. NAYLOR

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THE EARL OF LONGFORD and THOMAS P. O'NEILL. *Eamon de Valera*. Boston: Houghton Mifflin Company. 1971. Pp. xix, 499. \$12.50.

The authors present de Valera as a Christian democrat who has, throughout his long career, been preoccupied with lasting peace between Ireland and England, international amity, and politics in obedience to the Sermon on the Mount. He is compared with Gladstone and Lincoln, Sir Stafford Cripps and Robert Schuman, de Gasperi and de Gaulle. No evidence is adduced for these comparisons. The critical reader may wonder if de Valera has not had something to do with the sword as well as with peace.

When the events of de Valera's life contradict the presentation of him as Christian democrat, the narrative gets cloudy. This is especially true of the treatment of his willingness to engage in a civil war over the oath of allegiance to the Crown in the Anglo-Irish treaty, which had, after all, been accepted by a majority of the democratically elected Dáil.

Longford and O'Neill have produced hagiography. They maintain no distance from their subject; his self-deceptions are accepted as truth; his enemies become theirs. The achievements of the Cosgrave government in setting the new state on its feet and in using "the freedom to win freedom" conferred by the treaty are practically ignored. Modern Ireland was, it seems, the exclusive creation of Eamon de Valera.

There is no inquiry into motive because, as this account has it, de Valera had no motives. He had principles. Still, a psychohistorian might feel that certain events of his early life were important. When he was three, his father

died, and his mother, who had to go to work, put him out to be cared for. He was then shipped back to his grandmother in County Limerick to be brought up, and an aunt whom he had grown attached to, had to emigrate to America because of poverty. Might not these wounds to self-esteem, these frustrations of needs to depend, have evoked a fund of anger to be spent (not exclusively on England) and a determination on personal, and later national, independence? Longings to depend seem to have persisted, manifesting themselves in an intense and personal reliance on God. But he did not, perhaps could not, "inter-depend" with human beings. No one seems to have been close to him or to have known what he was really like, including the authors of this official biography.

This book does not erase the sense that de Valera was often rigid, narrow, negative, and life-denying. We know what he was against—external domination. What was he for? Keeping faith with the past and the dead, the restoration of the Irish language, an unpartitioned republic. But beyond that did he want to make the Irish people more prosperous, healthier, better educated? Hardly. His policy of economic nationalism wasted, according to less flattering accounts than this one, the considerable assets with which the new state began. The terrible poverty of the Irish in the 1930s, resulting from the economic war that he had provoked with Britain, and the continuing drain of emigration, which by the 1950s reached the highest rate since the agricultural depression of the 1880s, are facts that the authors choose not to emphasize. Their hero, in any case, was beyond "materialism." "The frugal fare of the cottage" he believed to be Irish, Christian, and uplifting. The republic required sacrifice. If an English government had been responsible for the hardship inflicted by his regime, the mind boggles at what the reaction in Ireland would have been. While he was leading his country toward economic disaster, he was denouncing partition, imposed and maintained by "English force," as the cause of Ireland's ills. The moral ambiguities of this career do not receive the attention they deserve here.

De Valera's place in Irish history is hardly in question. He belongs with the immortals, O'Connell and Parnell. He survived many dan-

gers and, most of all, he survived his own mistakes. Until a biography gives him to us warts and all and places him in the context of Irish society and politics, we will not understand how he did it or why the Irish people needed, wanted, and admired a leader like him.

JOSEPH M. WOODS
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HENRI TRIBOUT DE MOREMBERT. *La Réforme à Metz*. Volume 1, *Le luthéranisme, 1519-1552*. (Annales de l'Est, Mémoire, Number 36.) Nancy: Faculté des Lettres et des Sciences humaines de l'Université de Nancy. 1969. Pp. 229.

This exasperating book provides a striking example of both the strengths and weaknesses of a certain type of local history. Its subject is most promising. Metz was a free city of the Holy Roman Empire from 1519 to 1552 and hence subject to the seductive attractions of Protestantism to which so many of the free cities, notably its prosperous neighbor Strasbourg, succumbed. Yet Metz was also a French-speaking city in the province of Lorraine and thus subject to the formidable pro-Catholic pressures of the powerful Guise family backed by the Crown of France. Its situation on a linguistic, political, and religious frontier could make of its Reformation history a fascinating case study in the collision of rival cultural forces. M. Tribut de Morembert, director of the Metz Archives, is ideally placed to undertake such a study. Unfortunately he lacks, on this showing, the requisite analytical ability, the fund of general information, and a command of Reformation bibliography. He does present, to be sure, a good many fresh facts on the involvement of Metz in the Reformation, based on extensive research in manuscript repositories in Metz itself, in Strasbourg, and in Paris. Many of the manuscripts he uses are quoted *in extenso* with suitable precision. However, his fresh facts are sewed together with stale summary accounts of the contemporary history of the Protestant and Catholic Reformations and of imperial religious policies. This general information is drawn from a curious rag bag of scholarly essays, encyclopedia articles, and popular histories, many of them sadly out of date. It is often not related closely

to developments in Metz and, indeed, information on the operation of Metz's government, which is really needed for a full understanding of this story, is omitted. The resulting mixture may prove valuable to specialists, but it will have to be used with care.

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JOHN C. RULE, editor. *Louis XIV and the Craft of Kingship*. [Columbus:] Ohio State University Press. 1969. Pp. x, 478. \$12.00.

Professor Rule proposes in this book to explore some historiographical problems of the reign of Louis XIV. He brings together a dozen essays by distinguished scholars in the field, four of whom had prepared their papers for a conference at Ohio State University in 1964. Rule frames the essays with two of his own, one surveying the major events, the other the bibliography of Louis's reign.

Not all of the essays are true to the book's title. Only four consider the "craft of kingship" as such: Rule's one-hundred-page survey, which stresses the bureaucratic side of Louis's activities; both of John B. Wolf's essays, the first on Louis's preparations for kingship (the conclusions of which are similar to those of his fine biography of Louis), the second on Louis as a "soldier-king"; and A. Lloyd Moote's essay on Louis's concepts of law and justice as illustrated by his actual practices. Another four essays are more accurately discussions of the king's motives, and they are the most provocative of the volume. They include a revisionist essay by R. M. Hatton on Louis's foreign policy, an analysis of Louis's relations with the Church by H. G. Judge, a description by Andrew Lossky of the changing intellectual assumptions that lay behind Louis's actions, and a discussion by William F. Church of Louis's doctrine of reason of state. Four deal specifically with historiographical questions: Herbert H. Rowen tries to untangle the various meanings given by historians to the term "absolutism"; Paul Sonnino discusses interpretations of the king's *Mémoires*; Nathan Whitman reveals the meaning of the fountain of Latona at Versailles; and Orest Ranum provides a series of

reflections on the attitudes of Louis and his administrators toward the king's court and capital. One paper, by C. D. O'Malley, expertly and amusingly traces the medical history of the king, but its exact place in this collection is not apparent.

Rule claims in his preface that there is a surprising consensus among the writers on some of the historiographical issues involved, and he says he will make no effort to reconcile what differences there are. But the differences, even the subtle ones, result in a confusing portrait of the Sun King. For Rule, Louis is the bureaucrat *extraordinaire*, "modern" in his methods, aggressive in foreign affairs but willing to compromise in order to maintain stability, a great builder and patron. Wolf sees Louis as a "professional" in all his roles, plying his trade of kingship with self-awareness and a strong sense of security. In religious matters, Judge argues, Louis had a conscious aim: to maintain the purity of the faith; and he exploited every means to that end. From Church's essay we learn that Louis's concept of kingship, based on a widely accepted doctrine of reason of state, gave him great potential for despotism. Thus far we have a portrait of Louis as a confident man and monarch, one who knew what he wanted and who set out diligently, and with no mean ability, to get it.

But then turning to Hatton, Lossky, and Moote, we feel less sure. Hatton contends that Louis's foreign policy was defensive, that his goals for France were actually quite modest, and that he was "more frightened than has been realized." Lossky portrays Louis as a man with only a few consistently held ideas and traces a pattern of changing beliefs and motivations that makes it hard, if not impossible, to draw a tidy picture of him. Moote considers Louis's attitudes toward the law rather disorderly and inconsistent. Even Judge attests to the diversity of Louis's interests in religious matters. We are obviously dealing with a very different view of the Sun King. Louis appears here to be complex, changeable, and often influenced by outsiders and events, in a word, more human. The image of Louis as single-mindedly devoted to any goal, such as the pursuit of *gloire*, power, or territorial aggrandizement, not only contradicts the less "tidy" view

but is less convincing. Some comment by the editor on these matters would have been helpful.

ELISABETH ISRAELS PERRY
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FELICE HARCOURT, editor and translator. *Memoirs of Madame de La Tour du Pin*. With an introduction by PETER GAY. New York: McCall Publishing Company. 1971. Pp. 468. \$8.95.

It is very hard to agree with the publisher's claims that these memoirs are equal in interest to those of Madame de Staël and other woman authors of approximately the same period. These are too weak in insight and analysis, though quite as rich as most others in detail, anecdote, and description. The setting is that of the great French Revolution and the age of Napoleon, and the author is convinced that the vices of the aristocracy were responsible for bringing on the former, while she has only admiration for the power and efficiency of the latter. Hating deviousness, this author has small regard for Talleyrand, though they share much in their ability to survive, for Madame is a formidable survivor whether it be as courtier or short-term successful farmer in America. Often the hair's-breadth escape from peril and the ingenious schemes for preserving life and goods smack of Dumas and Hugo, but the account has enough apparent verisimilitude to be recommended as historically useful as well as occasionally amusing. The translation by Felice Harcourt is smooth and has every appearance of accuracy.

GEORGE T. ROMANI
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F. F. RIDLEY. *Revolutionary Syndicalism in France: The Direct Action of Its Time*. New York: Cambridge University Press. 1970. Pp. vii, 279. \$13.00.

PETER N. STEARNS. *Revolutionary Syndicalism and French Labor: A Cause without Rebels*. New Brunswick: Rutgers University Press. 1971. Pp. 175. \$9.00.

Historians have generally assumed that revolutionary syndicalism was the most striking characteristic of the French labor movement during the dozen years or so on the eve of

World War I. Was it, in fact? An important question, involving as it does relationships between theory and practice, between the leaders of a movement and its rank and file; and also a difficult one. It is not easy to define revolutionary syndicalism, and historians have disagreed. Was revolutionary syndicalism itself a doctrine (and, if so, whose doctrine)? Or a form of labor practice? Or, was it perhaps both? And how is the historian to measure the extent of its range, the intensity of its impact, its historical importance? This question, of the relationship between revolutionary syndicalism and French labor, is a common concern of the two quite different books here under review, though it is of particular importance for Professor Stearns.

Mr. Ridley is a professor of political theory at Liverpool, and his focus is upon revolutionary syndicalism itself, as expressed particularly through syndicalist newspapers like *Voix du Peuple* and the pamphlets of such militant theorists as Griffuelhes and Pouget, Yvetot, and Delesalle. For him revolutionary syndicalism is not a doctrine (such as Georges Sorel sought to provide), not even the doctrine of these active labor militants. It is at once less systematic—a range of ideas rather than an agreed body of doctrine—and broader. It is a movement rather than a theory, a general trend within the French labor movement, the principles and practice of the *Confédération générale du travail* (CGT) in the years between 1902 and 1914. Such a definition implies the integration of theory with practice and supports the traditional assumption that revolutionary syndicalism was indeed significant, even while Mr. Ridley does point out that the revolutionism of his militants was only one of several voices within the CGT and still less representative of French labor as a whole.

His book is an interesting one, clearly stated, carefully organized. The first of his three main divisions is an analysis of various factors in the history of nineteenth-century France that serve to account for the emergence of a revolutionary syndicalist leadership of French labor: the resistance of the state to trade-unionism and the latter's belated development; the revolutionism of Blanqui; Proudhon's old-fashioned, libertarian insistence on working-class autonomy; the anarchism of the 1890s; French So-

cialist collaboration with the bourgeois republic and the sense of betrayal such collaboration provoked; and the creation in 1895 and subsequent growth of the CGT. Part 2, the core of his monograph, is a discussion of the CGT under revolutionary syndicalist leadership and of such matters as the twofold purpose of reform and revolution; the theory of the strike and the strike in practice; sabotage, the boycott, and the label; antimilitarism and antipatriotism; the general strike in practice (and its diminishing role, as the leadership became less romantic and more realistic); and the general strike as myth. Finally, in part 3 Professor Ridley sets the revolutionary syndicalism expressed during these years within a broader ideological context characterized by a revolt against reason and a revolt against democracy. He discusses here the parallels between the views of Georges Sorel, in particular, and those of Nietzsche, Bergson, and William James—with which Sorel was in fact acquainted, though he came to them late and hence owed little to them—and between revolutionary syndicalism's critique of bourgeois democracy and that of France's nationalist and monarchist Right and Mussolini's fascism. Ridley finds them all reacting against an apparent failure of democracy and expressing a similar temperament—romantic, activist, and anti-intellectual.

Urging syndicalism's significance both because it dominated the French labor movement for two decades and because it offered a coherent, self-contained doctrine of socialism, he ends as he began by calling attention to the re-emergence of many of its ideas in May 1968 and thereby explaining his subtitle, "The Direct Action of Its Time."

This conclusion (or directing assumption) that revolutionary syndicalism dominated the French labor movement has now been challenged by an American social historian, Peter Stearns. His primary concern is the actual beliefs and practices of the French labor movement's rank and file, through which he might test the principles proclaimed by the syndicalist militants; and he has attempted to discover what these beliefs and practices actually were by means of an extensive analysis of the patterns of French strikes during syndicalism's heyday, from 1899 to 1913. His analysis is based upon an impressively broad variety of

materials, especially official statistical repertoires and archival reports on French strikes and union activities, and is buttressed by comparisons with German and British strike patterns during the period in question. Inverting Professor Ridley's approach much as Marx inverted Hegel's, he has approached the "extraordinary doctrine" of revolutionary syndicalism "from the bottom up" (p. 6). His point of departure is the anonymous mass of ordinary workers, whose readiness to strike he finds to have been brought about by economic causes rather by syndicalist doctrine.

Professor Stearns has concluded that there was in fact a coincidence between a rise in the strike rate and the advance of syndicalism, but that this coincidence was not significant. Syndicalism was not a factor in the variation in strike demands and tactics from one industry to the next. The reformist miners of the north were more violent than the syndicalist miners of Saint Etienne, and even where syndicalism and violence were associated, as in the prolonged series of strikes of the construction workers at Paris, economic factors were the real cause. Various economic factors are suggested in the course of the essay. Of particular interest, and emphasized by the author, is a decline in real wages from about 1901 through 1910 (p. 116). The French workers were essentially moderate, and their moderation domesticated their syndicalist leaders, rather than the reverse. Violence was no more common in France than it was in England or Germany, and general solidarity strikes (such as one might expect from the principle of the general strike) were much rarer. Revolutionary syndicalist rhetoric not only failed to inspire French labor, it harmed it—by so shocking "innumerable French workers" that they rejected the labor movement altogether, by provoking a new toughness on the part of government and employers and the hostility of the public, and, possibly, by stalling parliamentary efforts toward social reform through the fear that syndicalist pronouncements produced. Though he adds, with seeming inconsistency, "neither the government nor the employers took syndicalism too seriously." Its importance is a myth.

Is this argument convincing? Professor Stearns fails to make sufficiently explicit the criteria by which he is gauging the insignifi-

cance of syndicalism, and this is unfortunate because syndicalism professed to be a philosophy of labor practice and did in fact have much in common with it, including the ordinary strike for higher wages. Moreover, he does not develop in a systematic fashion his crucial comparison between the behavior of syndicalist unions and nonsyndicalist ones. He even writes, "We can never know for sure [who was really a syndicalist], but in terms of protest activity, it does not seem to matter much" (p. 97). But, if we cannot know for sure who was a syndicalist, how can we be sure that this distinction does not matter?

But, though he does not "prove" the lack of influence of the militants upon the behavior of the rank and file, he may be right all the same. There almost certainly was a great distance between militants inspired by revolutionary principles and ordinary workers striking exclusively in behalf of immediate goals, as was recognized by the syndicalist leaders themselves as well as by Georges Sorel. Hence their elitism, their insistence on the task of leadership incumbent upon the militant minority. This distinction also helps to account for the divergence between Ridley and Stearns in judging the significance of revolutionary syndicalism. It was of at least doubtful importance as an inspiration of the mass of French labor, as Stearns contends. But it was significant, on the other hand, as a social philosophy, voiced by its militants, that reflected a combination of historical and institutional conditions peculiar to early twentieth-century France.

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ALAIN TOURAINE. *The May Movement: Revolt and Reform. May 1968—the Student Rebellion and Workers' Strikes—the Birth of a Social Movement*. Translated by LEONARD F. X. MAYHEW. New York: Random House. 1971. Pp. 373. \$8.95.

A. BELDEN FIELDS. *Student Politics in France: A Study of the Union Nationale des Étudiants de France*. (Student Movements—Past and Present, Number 5.) New York: Basic Books. 1970. Pp. viii, 198. \$7.95.

"I have something to say," a student wrote on the walls of Paris during the "revolution" of 1968, "but I don't know what it is." Alain

Touraine's book is the most imaginative attempt to bring the message to consciousness, to do for revolutionary students what Marx did for industrial workers, to close the gap between theory and practice and unite the student movement with New Left sociology. The work is neither history nor sociology (there is no documentation); it is the sociological imagination in search of the meaning of a historical event. Touraine attempts to separate what was elemental and international in the revolution from what was historically conditioned or uniquely French. The resulting force is called the "May Movement." In answer to the commandment of industrial society, "Adapt yourself," the revolutionaries replied, "Express yourself." In a word, the May movement was the familiar revolt against technocracy, the rising of the managed against the managers. But Touraine adds much more: the result is the best radical interpretation of the French revolt presently available.

"The May Movement," Touraine contends, "fought the past and revealed the future." It stormed archaic institutions and unmasked a new social enemy. And the enemy was everywhere: it could not be personalized for it was depersonalization itself, a pervasive system of authority over human life, an anonymous power advancing in the name of economic rationality and social "progress." The insurrection exploded outward from the center of society—students, technicians, professionals, and young workers—united in a wave of populism and life. They were a revolutionary movement in a nonrevolutionary situation, a cultural revolt in a nation prosperous, integrated, and industrialized. Ultimately they lost their way, some retrogressing into traditional political expressions and others becoming isolated in an "anti-society" behind barricades, consuming the revolution in talk. But their experience, Touraine believes, marked the end of the liberal illusion that industrialization deadens the roots of revolution. "The May Movement," he concludes, "was a thunderbolt announcing the social struggles of the future."

Professor Fields holds to the liberal "illusion." His brief study of the decline of the Union Nationale des Étudiants de France during the 1960s has convinced him that stable societies such as France and America have little

reason to fear the revolutionary activity of students. Where Touraine is preoccupied with the higher levels of student consciousness, Fields is concerned with cafeterias, money, and square feet, and his chapters on the student condition in France are the most interesting sections of this rightfully modest work. It is offered in the hope that the French experience will aid Americans to understand their own student protests, but readers will become convinced that the situations are too different for such unhistorical comparisons.

The appeal of Fields's book is not its arguments but its subject: the revolt of the young arouses interest these days. Nor is Touraine's work likely to convince the unconverted; despite its intellectual flair it is manifestly a political point of view. The attraction of both books is not that they are convincing but that they are fashionable.

GILBERT ALLARDYCE

University of New Brunswick

MARVIN LUNENFELD. *The Council of the Santa Hermandad: A Study of the Pacification Forces of Ferdinand and Isabella*. Coral Gables, Fla.: University of Miami Press. 1970. Pp. 134. \$5.95.

The Santa Hermandad (Holy Brotherhood) was a league of the Castilian Crown and towns created in 1476 ostensibly for the purpose of suppressing brigandage and crime in the countryside. Traditionally it has been viewed also as a weapon of royal intimidation to bring the landed nobles into line. This rural police force exercised jurisdiction over many types of crime, including housebreaking, rape, murder, and robbery committed in the country or in small villages, and acts of rebellion against the government of Ferdinand and Isabella. As a direct agency of the Crown, this organization sent a tremor through late fifteenth-century Castile. It was supposed to have struck terror especially into the hearts of the *grandees*, for the Hermandad archers had access to every refuge or asylum in the kingdom. Even the strongholds of the mighty were subject to the vicious justice of the Brotherhood and, through its governing council (appointed by the queen herself), to the prying eye of the central government.

According to Marvin Lunenfeld, this image of the Santa Hermandad is not entirely correct. The Brotherhood was feared in Castile, but

more because of its ruthlessness than because of its royal affiliation, which was, in effect, rather loose. From manuscript sources at Simancas, Madrid, Seville, Burgos, and Toledo, and from a cross-section of printed documents, Lunenfeld demonstrates—convincingly, on the whole—that the Brotherhood was neither an instrument of governmental centralization and unification nor a scourge to the nobility. Its main thrust, in fact, was directed—with aristocratic support—against rural lawlessness and foreign threats.

The municipalities themselves were not uniformly enthusiastic about the institution. It was resisted by many of the towns, some of whom refused to pay the taxes for its upkeep. It succeeded largely because of the Portuguese invasion during the Castilian succession crisis of 1475–79 and the exigencies of the Granada war (1482–92). The council of the Brotherhood, loosely organized and much less powerful than is usually assumed, was disbanded in 1498 after these crises were favorably concluded. Police activity then returned to local control as before.

The Santa Hermandad, however, did serve the more limited purpose for which it was founded, namely rural pacification during a critical and vulnerable period in Castilian history. Beyond that, the council, through its creation of a brotherhood militia to assist in the conquest of Granada, laid the foundation for a mobile royal infantry that later contributed much to Spanish military success.

DE LAMAR JENSEN

Brigham Young University

JUAN ROA DÁVILA. *De Regnorum Iustitia: O el control democrático*. Critical bilingual edition by LUCIANO PEREÑA, with the collaboration of J. M. PÉREZ PRENDES and VIDAL ABRIL. (Corpus Hispanorum de Pace, Number 7.) Madrid: Consejo Superior de Investigaciones Científicas, Instituto Francisco de Vitoria. 1970. Pp. liv, 215. 350 ptas.

This volume is another in the series of the Corpus Hispanorum de Pace, which has already turned out volumes devoted to the thought of Francisco Suárez (1965), Francisco de Vitoria (1967), and others of the notable school of Spanish theorists on the state of the sixteenth century. It is, I think, uneasy in such exalted company.

Juan Roa Dávila studied law at the Univer-

sity of Salamanca, entered the Jesuits in 1569, and finally, after leaving that order, became by royal appointment in 1593 prior of the house of the Augustinian canons of San Juan de Cavero in the province of Coruña. He was an "involved" cleric for his day, defending on theological grounds the Spanish conquests of the Indies, the practice of bullfighting, the legend of the preaching of St. James the Great in Spain, the proposed definition of the dogma of the Immaculate Conception, and the rights of the Spanish government to protect its subjects against the abuse of ecclesiastical jurisdiction. The latter drew him into the struggle between the Crown and the papacy in the late sixteenth century where he became expendable. Tried by the Inquisition, he appealed to Rome and spent the last years of his life there (1595-1630).

The book contains portions of the work published in 1591, *Apologia de iuribus principalibus defendendis et moderandis iuste*, condemned by the Inquisition in 1594. The text is based on the only edition and is accompanied by a Spanish translation and a documentary appendix relating largely to his subsequent troubles.

Roa Dávila held, on scriptural grounds, to the natural freedom of man, the origin of political power in a contract that limited all subsequent actions of the executive, the right of the people to be consulted on taxes, and their ultimate right of revolution against tyranny. He held as well to the traditional limitations of the Thomistic school on the conditions for a just war. In fact, his mind was traditional enough to admit "reasons of state" to justify exceptions to most of these positions, to accept the right of God acting through the papacy to depose kings, and to find war to advance religion just.

I find it difficult to detect any novel or outstanding contribution to political thought in the work.

BERNARD F. REILLY
Villanova University

J. G. VAN DILLEN. *Van rijkdom en regenten: Handboek tot de economische en sociale geschiedenis van Nederland tijdens de republiek*. The Hague: Martinus Nijhoff, 1970. Pp. ix, 698.

This book forms a worthy though posthu-

mous crown to Van Dillen's long and productive life (1883-1969). It is, in essence, a synthesis of the economic and social history of the Dutch Republic, 1580-1795. The end product of a lifetime of wide and deep reading (and of fruitful teaching), it presents a balanced overall survey, without indulging in sweeping generalizations or neglecting significant details.

The first section, "The Republic in Flower, 1580-1650," begins with an introductory chapter on Dutch economic growth in the late sixteenth and early seventeenth centuries, when the volume of the Dutch Baltic trade doubled between 1585 and 1620, and the production of textiles at Leiden doubled between 1595 and 1619. Other chapters in this section deal with the influx of Spanish-American silver and its role in Dutch maritime trade; the interdependence of Dutch trade with the Baltic, the Mediterranean, and Russia; the rise of the Dutch East and West India Companies; the vicissitudes of the textile industries of Leiden and Haarlem; and shipbuilding and the fisheries. Van Dillen writes with particular authority in the chapters on the monetary system, the Exchange Bank, and the Republic's finances, being the acknowledged master in all these fields. He does not ignore the widespread poverty among many sections of the lower classes during the "Golden Century." The textile workers of Leiden, for example, seem to have fared little better than the Amerindian laborers in the *obrajes* or textile sweatshops of colonial Spanish America.

The second section, "Consolidation and Decline, 1650-1740," analyzes the period in which the Dutch economy was at first characterized by a high degree of stability, the decades 1650-80 being reasonably prosperous on the whole, and the years 1680-1700 rather more so. Dutch participation in the War of the Spanish Succession (1702-13) inevitably had unfavorable repercussions on many but by no means all sectors of the economy. A rather belated return of commercial prosperity in the 1720s reached a peak in 1730-33, but industrial stagnation and decline became very serious at Leiden and elsewhere. Inflation and a fall in real wages meant that the gap between rich and poor became even more noticeable than in the seventeenth century. Social protest and unrest increased, though not very effectively from a working-class viewpoint.

The last section, "The Process of Contrac-

tion, 1740–95,” explains how the depression of c. 1735–c. 1760, was followed by an Indian summer of prosperity for trade (as distinct from industry) in 1760–80. Even the disastrous Anglo-Dutch War of 1780–83 did not spell the economic ruin of the Netherlands, as many writers, including myself, have maintained. Van Dillen argues that the economic situation of the Dutch Republic in 1795 was bad but not disastrous. He claims that the real death blow was given by the imposition of Napoleon’s Continental System. Readers familiar with Van Dillen’s previous work, and with the books of Johan de Vries, Fritz Snapper, Violet Barbour, Charles Wilson, *et al.*, will not find anything very new or startling in *Van Rijkdom en Regenten*. All those writers relied heavily on Van Dillen in their discussion of the Dutch economy; and he in his turn kept himself abreast of their work and incorporated it in his own. But readers of Dutch will be glad to have in this form the master’s last thoughts on the topics to which he devoted a lifetime. The book is rounded off by an excellent bibliography and an adequate index. Since Van Dillen died before the book was quite finished, the present work was prepared for publication by Mr. W. M. Zappey. The editorial task has been deftly and unobtrusively performed; but the overlap may account for a few slips such as the erroneous mention of Egypt where Persia is intended (p. 134), and of Makassar for Malacca (p. 137).

C. R. BOXER

Yale University

HEIKKI KIRKINEN. *Karjala idän ja lännen välissä*. [Karelia between East and West]. Volume 1, *Venäjäin Karjala renessanssijalla (1478–1617)* [Russian Karelia during the Renaissance Age (1478–1617)]. (Historiallisia Tutkimuksia, jalkaissut Suomen Historiallinen Seura, Number 80.) Helsinki: Kirjayhtymä. 1970. Pp. 344.

Professor Heikki Kirkinen’s *Karjala idän ja lännen välissä*—the second in a projected trilogy of studies dealing with Karelia (the first volume, *Karjala idän kulttuuripiirissä*, appeared in 1963 and was reviewed in this journal [70 (1964–65): 843])—covers Russian Karelia’s cultural history during the period of the Renaissance. Based on extensive archival research,

in Helsinki, Kuopio, Stockholm, Leningrad, and Moscow, as well as on a wide array of published materials, it is a notable achievement and testimony of Kirkinen’s oft-expressed conviction that the study of Karelian history must be liberated from long-existent Western (Finnish and Swedish in particular) perspectives, that greater use be made of Karelian, Russian, and Slavic resources, and that historians, in short, ought “to try to understand how the world looked to the Karelians themselves and how they viewed their own lives” (pp. 6–7).

The author examines in rich detail the patterns of settlement; government; agriculture, industry, and trade; folk culture; religious institutions, beliefs, and practices. Very informative are the descriptions of Karelian monasteries (among them Solovetski, Konevitsa, and Valamo), the ascetics and holy men, some of whom attained national prominence, and hagiographic literature. Folklorists will find in Kirkinen’s well-documented thesis that the Finnish national epic, the *Kalevala*, contains much that is indigenously Karelian (as well as elements drawn from Russian, Slavic, and worldwide sources) a useful corrective to the widespread misconception that the poem’s exclusive birthplace was West Finland. Kirkinen’s major conclusions are summarized in a French language resumé.

For Karelia, the age of the Renaissance was one of remarkable economic progress, considerable autonomy, lively religious activity, and a fruitful interaction between traditional Finnish-Karelian, pagan, and Kalevalan foundations and influences emanating from Byzantine, Russian, Slavic, and Orthodox origins, creating what in Kirkinen’s judgment was “an amazingly original, rich, and integrated Karelian culture” (p. 252).

The perennial and major threat to Karelia’s continuing development was Swedish-Russian imperial rivalry, especially during the decades following 1570. How and in what manner Karelian culture survived the havoc and destruction of almost uninterrupted warfare is the theme of Kirkinen’s promised third volume. This reader, for one, eagerly awaits its publication.

JOHN I. KOLEHMAINEN
Heidelberg College

YELLA ERDMANN. *Der livländische Staatsmann Johann Reinhold von Patkul: Ein abenteuerliches Leben zwischen Peter dem Grossen, August dem Starken und Karl XII. von Schweden*. Berlin: Haude & Spencersche Verlagsbuchhandlung. 1970. Pp. xii, 309. DM 48.

Baltic history has much to offer those historians searching for fresh topics and unusual historical personalities. Yella Erdmann has been particularly attracted by the life and personality of one of the principal instigators of the Great Northern War, the Baltic German baron, Johann Reinhold von Patkul, who played a considerable role in the courts of Sweden, Saxony-Poland, and Russia. Erdmann, a historian of Baltic German origin herself, had originally planned to write a novel. Patkul's bizarre life is indeed a most suitable subject for a psychological novel with historical trimmings. After perusing piles of almost untouched source materials Erdmann decided to write a historical monograph. Her book is well written, very interesting, and generally objective. Unfortunately it still looks and reads more like historical fiction. She has used extremely good sources, many of which are no longer available after World War II, but has decided not to use footnotes in order "not to disturb the reader." Her bibliographical lists also leave something to be desired. At times she lets loose her biases, for example, when she is writing about the Swedish king Charles XI and his governor-general in the Baltic provinces, Jakob Johann von Hastfer. Whenever she mentions the independent Baltic states she uses quotation marks. To her the Baltic countries were a "German colony." She is also somewhat weak in legal analyses.

One should say, however, that the development, explosion, and degeneration of Patkul's personality is objectively and clearly depicted. Erdmann has also done a good job in her projection of a wide historical panorama. She seems to feel quite at home in the immensely complicated arenas of clashes of personalities or national interests of various powers, political and military moves, plots and counterplots, and double and triple games. She is able to lead her reader through the maze of intrigues during one of the most chaotic periods of European history without leaving too many questions unanswered. This was the time when the

Russians finally managed to establish a foothold on the Baltic coast and to start to interfere in European affairs. Twelve photographs of rare portraits and documents are added to the volume.

EDGAR ANDERSON

San Jose State College

VAGN DYBD AHL. *Partier og erhverv: Studier i partiorganisation og byerhvervenes politiske aktivitet ca. 1880-ca. 1913* [Political Parties and Trades: The Organization of the Danish Political Parties and the Political Activities of the Urban Population and the Trade Organizations, 1880-1913]. In two volumes. Aarhus: Universitetsforlaget. 1969. Pp. 406; 235. 90 D. kr.

In Scandinavia, as in other countries, there has been in recent years a tendency to make greater use than was customary in the past of the tools of analysis developed by social scientists, particularly political scientists. Several interesting attempts have been made to analyze popular political participation, often as represented in local and national elections. Dybdahl's dissertation, replete with maps, tables, and diagrams and defended in 1969 at the University of Aarhus (where another historian, Jørgen Weibull, has also employed similar methods), is an example of this approach; and it differs markedly from the traditional political and diplomatic interests of his mentor, the late C. O. Bøggild-Andersen. Aside from a thorough examination of newspaper and periodical literature and printed primary and secondary sources, he has employed materials from party archives, city archives, provincial and national archives, and the commercial archives Erhvervsarkivet (Aarhus), of which he has been head archivist since 1948. His concluding chapter and summary in volume 1 (pp. 387-406) has been translated into English in volume 2 (pp. 5-27). The English reader should proceed with some caution for the translator has had difficulties. Many are connected with the Danish word *erhverv*, cognate with German *Erwerb*, which has an unfortunately large number of English equivalents depending on how it is used (occupation, trade, pursuit, industry, employment, profession, business, and so forth). Dybdahl aims to determine how the urban tradesmen and businessmen were organized within the parties and in

political clubs associated with the Conservatives or the Social Democrats, how they voted, and how they attempted to bring about legislative changes. The heavy support of the Conservative party by government officials (during this period more than four-fifths) and by the professional classes—doctors, lawyers, teachers, academicians, and so forth—is not unexpected but the support of such a large number of the working classes is perhaps surprising. It should be remembered, however, that the Danish constitution of 1866 disenfranchised a large percentage of the poorer portions of the urban working class. In his treatment of the Conservatives, Dybdahl emphasizes analogies with the English Conservatives, who also did not consider themselves a class party. Social clubs played an important role in this. Where the Social Democrats are concerned, the local and national characteristics rather than the international are emphasized with little attention to Marxist dogma. Lieutenant Colonel Wagner speaking for the Conservatives in 1895 (vol. 1, p. 398) commented that they could easily be both protectionists and free-traders, supporters or opponents of cooperatives. Denmark, in these as in other respects, was an exception.

In his analysis Dybdahl has deliberately avoided any consideration of basically non-economic questions, such as laws on education, and defense policy, for example, but he is convinced that his work will lead to further research on the basis of the strength of the Estrup government (1875–94), especially during the period 1884–93, when the Conservatives governed without a budget. This is a fresh and worthwhile new approach to Danish political history.

ERNST EKMAN
*University of California,
Riverside*

SVEND AAGE HANSEN. *Early Industrialisation in Denmark*. (Københavns Universitet, Institut for Økonomisk Historie, Publication Number 1.) Copenhagen: G. E. C. Gads Forlag. 1970. Pp. 77. \$3.00.

The author, who explicitly prefers the term “early industrialization,” to the time-honored but less useful “industrial revolution,” attempts in this relatively unpretentious work to give a Danish dimension to Rondo Cameron’s *Banking in the Early Stages of Industriali-*

zation (1967). He undoubtedly achieves his objective, although in the process he establishes the fact that banking institutions did not play such an important a role in promoting and financing the growth of Danish industry. General historians may be somewhat startled to learn that for Hansen “early industrialization” in Denmark means the 1890s, but economic historians who follow the W. W. Rostow dispensation and its refinements will have little difficulty accepting the thesis. Although Denmark had factories of one type or another in virtually every small town long before 1890 and industrial clusters in all the larger towns, industry’s share of the aggregate gross domestic product even in 1890 was only seven per cent. The productiveness of Danish agriculture and the flourishing of commerce (heavily associated with export-import activity) help to account for the peculiarities of the industrial breakthrough that followed, but Hansen provides us with much detail hitherto accessible only to those familiar with the Danish literature. His principal contribution in the narrower scholarly sense is, of course, his fairly comprehensive review of the role played by the various types of financial institutions and the characteristics of the Danish money market that influenced the industrial development of the 1890s.

HEINZ E. ELLERSIECK
California Institute of Technology

ERKKI LEHTINEN. *Suomen varhaishistorian ja ristiretkikauden kuvasta uskonpuhdistus- ja suurvalta-aikana* [Concerning the Image of the Early History and the Time of Crusades of Finland during the Reformation and the Great Power Period]. (Historiallisia Tutkimuksia, Number 75.) Jyväskylä: Suomen Historiallinen Seura. 1968. Pp. 194.

This is a historiographical study that concerns itself in particular with the historical image of Finland as it is reflected in the historical literature of sixteenth- and seventeenth-century Sweden and Finland. The author is interested in the possible connection between this image and a sense of Finnish national identity that, he argues, already at this early period evolved into something distinct from a dominant identification with the Swedish state. In emphasizing the connection between historical writing and the need for national aggrandizement and the urge to rebut nagging hidden feelings of inferiority,

Lehtinen has abundantly juicy material, although most of it is already familiar to historians. While the author has utilized the most important literature pertaining to his topic, he has not explored all the available original manuscripts and different variations of texts that might have enabled him to trace in detail the changes taking place in interpretation and argumentation.

What then are his findings and conclusions? On one hand Lehtinen duly underlines the continuity of the narrative material and, to a lesser extent, interpretation between the late Middle Ages and the early modern age. On the other hand he appropriately notes the deep influence of German Lutheranism and humanism upon historical writing in Sweden and Finland. He emphasizes that historical writing was primarily treated as and understood to be a fund for providing guiding material for "statecraft and morality"—without, however, analyzing clearly how it fulfilled such a dual role and what approach dominated the actual writing. The historians' arbitrary way of handling sources in an effort to satisfy and please contemporaries and their primitive world view were, in Lehtinen's opinion, the very reasons that accounted for the appeal and influence of their products.

Lehtinen has chosen to organize his book on an author-by-author basis, which allows him to state clearly the individual author's views, but it also creates an unnecessary amount of repetition when the same issues are touched on several times. It would perhaps have been easier to follow the changes taking place in the description and interpretation of the most crucial historical events and issues if the organization had been topical, but this is a matter of individual taste. While the material used in the preparation of the book is for the most part well known, Lehtinen does bring it together for the first time and has treated it in a sensible and knowledgeable manner. He has made a welcome contribution to Finnish historiographical literature.

PEKKA KALEVI HAMALAINEN
*University of Wisconsin,
Madison*

AARNE MATTILA. *Työmarkkinasuhteiden murros Suomessa: Tutkimus työntekijäin pyrkimyksistä osallistua työehtojen määräämiseen 1880-luv-*

ulta vuoden 1905 suurlakkoon [Transition Period in Finnish Labor Relations: A Study of the Efforts of Employees to Gain Participation in the Determination of the Terms of Labor Contracts from the 1880s to the General Strike of 1905]. (Historiallisia Tutkimuksia, Number 76.) Helsinki: Suomen Historiallinen Seura. 1969. Pp. 299.

Although much recent attention has been given by historians to various aspects of the development of a working-class political movement in Finland, research on the emergence of early Finnish labor movements has been limited. This shortcoming has undoubtedly been due in part to the enormity of the task of sifting through the fragmentary records of local trade unions and of achieving a balanced synthesis based upon a vast but uneven accumulation of primary source materials. In the study under review, Dr. Mattila attempts just this task. By examining closely the actual process whereby economic conditions influenced the development of labor-management relations, Mattila deepens substantially our understanding of the Finnish working-class movement at the turn of the century.

Mattila's book concentrates on the attempts by the young trade unions to achieve a position of parity vis-à-vis Finnish employers in contractual negotiations. Here Mattila restricts his attention to the period from the 1880s through the general strike of 1905. The transition he describes involves a movement away from trade associations composed of workers and the more liberal employers toward a sharply polarized labor market in which workers' unions, skilled in the application of collective pressure and strike activity, had virtually accomplished the abolition of employer-dictated relations. Mattila finds that it was usually the skilled artisan groups in the larger towns engaged in such industries as printing, metalworking, and construction that took the lead in this new development. Most large-scale factory industries lagged behind, partly because of their heavy reliance on the migration of poorly skilled workers from rural areas, for whom even low factory wages were an improvement over the irregular income of seasonal employment.

Although the author is also concerned with the way in which employers responded to the challenge posed by increasingly aggressive labor unions, he has clearly encountered great difficulty in finding comparable source material re-

flecting the long-range policies of the less cohesive employers' associations. In this connection, it is somewhat puzzling that he appears to rely more heavily on the "Old Finn" newspaper, *Uusi Suometar*, than on such Swedish language papers as *Hufvudstadsbladet* or *Nya Pressen*, which might have expressed better the opinions of the predominantly Swedish-speaking class of factory owners.

Mattila tends to be overcautious in formulating his conclusions. Nonetheless, he must be commended for accomplishing a thorough and well-disciplined piece of research on a rather unwieldy topic. What is more, his attempt to set Finnish developments in the broader context of contemporaneous movements elsewhere in Europe widens the appeal of the book beyond the horizon of merely the specialist in Finnish affairs.

FREDERICK J. AALTO
Indiana University

KONRAD FEILCHENFELDT. *Varnhagen von Ense als Historiker*. Amsterdam: Verlag der Erasmus Buchhandlung, 1970. Pp. 381.

Karl August Varnhagen von Ense has generally been regarded as an essayist and biographer important not for his limited literary talent but for his acquaintance with the leading figures in the cultural and political life of Germany during the first half of the nineteenth century. He knew in varying degrees of intimacy all the luminaries of his age, so that his memoirs, diaries, and letters have been a valuable source of information regarding prominent personalities whose claim to immortality is greater than his own. Although he wrote romantic poems and esthetic critiques as well as biographical studies and political treatises, none of them has been considered better than second rate. His name has appeared most frequently in the footnotes and bibliographies of works dealing with his more famous contemporaries. But now Konrad Feilchenfeldt has published a book in which Varnhagen occupies the center of the stage, not as a contributor to literature or a participant in politics, but as a writer of history. The man who has usually been remembered for what he had to say about others is now himself the protagonist of a serious work of scholarship.

Since Varnhagen was not essentially a histo-

rian, the book has to analyze his view of history on the basis not only of the few historical works he did write, but of his memoirs, diaries, and letters as well. It concentrates on the period of the War of Liberation and the early Restoration, with which most of his books on history deal. But there are also chapters on the revolution of 1848, on Varnhagen's biographical studies, and on his general view of the historical discipline. All of this has been done with painstaking labor and scholarly thoroughness buttressed by detailed footnotes and impressive bibliographies. And yet a gnawing doubt remains that the result may not have been worth the effort. As the author himself points out, Varnhagen was "not only no professional writer of history, but also no historical thinker." The traditional view of him as a talented dilettante and gossip is perhaps not entirely unjustified. For not even Feilchenfeldt's great learning and meticulous scholarship will persuade all readers that Varnhagen was more than a mediocre historian who was also a mediocre poet and a mediocre essayist. This book should prove of interest to specialists in the cultural history of Germany during the early nineteenth century, but not to many others.

THEODORE S. HAMEROW
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Madison*

ERNST LUDWIG VON GERLACH. *Von der Revolution zum Norddeutschen Bund: Politik und Ideengut der preussischen Hochkonservativen, 1848-1866*. Volume 1, *Tagebuch 1848-1866*; Volume 2, *Briefe, Denkschriften, Aufzeichnungen*. Edited with an introduction by HELLMUT DIWALD. (Deutsche Geschichtsquellen des 19. und 20. Jahrhunderts, Number 46/I and II.) Göttingen: Vandenhoeck & Ruprecht, 1970. Pp. 484; 486-1399.

A previous selection of documents from the papers of Ludwig von Gerlach for 1805-20 (see Klaus Epstein's review of Hans Joachim Schoeps, ed., *Aus den Jahren preussischer Not und Erneuerung* [1963], in *AHR*, 70 [1964-65]: 448-49) gave fascinating insight into what this *Nachlass* still contains for the historian. Of even broader interest and value is the much larger collection for 1848-66 now elegantly produced under the auspices of the Bavarian Academy of Sciences. It brings to life the group of not-always-stuffy archconservatives whose

particular trademark was a blend of Protestant piety and visionary notions of a "true" Prussian tradition.

The cast of characters in these volumes is large, but the brothers Ludwig and Leopold von Gerlach occupy the center of the stage. The first volume belongs to Ludwig; it contains nearly four hundred pages of new material from his diaries. Although they were not written with an eye toward publication, and are often cryptic and elliptical, they nevertheless draw on the reader, as good diaries do, and fix his attention. The second volume contains almost a thousand pages of letters. A relatively small number of these were written by Ludwig, but there are nearly three hundred from Leopold to Ludwig. Until his death in 1861 Leopold dominates this volume. The Gerlachs were rabid letter writers, practitioners of the cut-and-thrust; and although Leopold and Ludwig were generally in agreement on fundamentals, they argued incessantly over details. Ludwig's diary and Leopold's letters, read together, give cohesion to the first big segment of the collection. As filtered through these pages the scene seethes with tensions, as conflicts develop among close friends of former years—with Radowitz, with Moritz Bethmann-Hollweg, and above all with Bismarck (for whose son Herbert, Ludwig had stood godfather). At first Bismarck wanders in and out of these pages dropping sharp-tongued comments on all and sundry, but after 1861 it is Ludwig's growing antagonism and finally his break with Bismarck that provides the main dramatic thread.

In 1848 the Gerlachs entered into intense political activity. By this time, Leopold, fifty-seven years old, long an intimate friend of Friedrich Wilhelm IV, had become a leading member of the king's military household, and not long thereafter was named *Generaladjutant*. In this position, one of the most influential in Prussia, he remained until Friedrich Wilhelm IV's death. Leopold was a political general par excellence. Ludwig, five years younger, president of the court of appeals at Magdeburg, was widely known by 1848 for his polemical journalism in the religious press and for the combative application of his religious and political principles to juridical problems. In 1848 through the Camarilla, the Gerlachs sought with partial success to exploit their in-

side position with the king to set the course of Prussian policy. They were leaders in founding the Conservative party and the *Kreuzzeitung*, and continued to press their points of view through these vehicles during the next decade and beyond. Their relations with Friedrich Wilhelm IV were by no means always smooth: toward his brother, the future Wilhelm I, their attitude was one of deep distrust.

The editor of these papers, Hellmut Diwald, has contributed a first-rate introduction, almost monographic in length. He was confronted, however, with a difficult problem in editing Ludwig's diaries, because in 1903 an heir of the family had drawn extensively on them for two volumes entitled *Aufzeichnungen*. The imperfections of this publication, though it has been widely used by scholars, have been long evident. Nevertheless, as Diwald ruefully acknowledges, he was prevailed upon not to duplicate entries previously published, despite the fact that this earlier work is not even to be found in every German university library. Consequently, what we now have are two fragmentary selections from Ludwig's diaries of the period 1848–66. A further imperfection in this otherwise well-edited collection is the absence of adequate identification of even some of the principal figures. The index is intended to serve this purpose, but the biographical information is too meager, and what there is, is not always accurate.

PAUL R. SWEET

Michigan State University

HERMANN JOSEF DÖRPINGHAUS. *Darwins Theorie und der deutsche Vulgärmaterialismus im Urteil deutscher katholischer Zeitschriften zwischen 1854 und 1914*. Freiburg: [the Author.] 1969. Pp. ix, 282.

This is a painstaking and discriminating study of German Catholics' efforts to come to terms with Darwinism and positivism. It resumes and analyzes the discussion of evolutionary theory, and of scientific issues and philosophy in general, which appeared in several leading Catholic journals of broad circulation between 1854 and 1914, dwelling especially on the clash between "vulgar materialism" (identified with the position of such writers as Ludwig Büchner and Ernst Haeckel) and a "spiritual-religious" interpretation of nature. The research is thor-

ough, and the interpretation illuminating; the organization is adequate, though over schematized in the hallowed style of German doctoral dissertations. The author's conclusion—excellent as far as it goes—is too brief to restore unity and interrelatedness to the ideas of either side.

Dörpinghaus's most valuable contribution probably lies in his having shown that Catholic writers mostly failed (especially in the early decades of the debate) to distinguish evolution as a principle of scientific explanation from the atheistic world view of Darwin's most prominent German popularizers; the ironic result was that down to around the turn of the century German Catholic spokesmen tended to agree with their opponents that there is a necessary and ultimate antagonism between science and faith. This had the effect of confining Catholic thought on science to a stubborn and largely futile negativism, a defensive intransigence that was marked by chronic unwillingness to listen attentively to opposing arguments or even to be well informed about the facts and issues in dispute. This posture did become less rigid toward the end of the period under review, anticipating the much greater changes that have since occurred, but not enough to restore effective communication before the coming of the First World War. Dörpinghaus fittingly ends with an ironic remark of Hans Küng: "Only when compelled by the science of others, which we have accepted superficially and unwillingly, only with many 'ifs' and 'buts' do we finally concede—often when the question is no longer of much interest for the rest of the world—that we are indeed not completely certain, or perhaps that we were not actually in error though we were not entirely right either."

RALPH H. BOWEN

Northern Illinois University

JOSEF BECKER *et al.* *Das kaiserliche Deutschland: Politik und Gesellschaft 1870–1918*. Edited by MICHAEL STÜRMER. Düsseldorf: Droste Verlag, 1970. Pp. 447. DM 42.

HANS-ULRICH WEHLER. *Krisenherde des Kaiserreichs, 1871–1918: Studien zur deutschen Sozial- und Verfassungsgeschichte*. Göttingen: Vandenhoeck & Ruprecht, 1970. Pp. 437. DM 34.

These two books form part of the voluminous literature that has been devoted to a re-evalua-

tion of the German Empire on the centennial of its founding. They are mostly the work of young West German scholars interested in social and economic history and ready to challenge many of the assumptions of their elders reared on a diet of political and intellectual history.

The symposium edited by Michael Stürmer provides convenient surveys of the present *Stand der Forschung* on such problems as the Empire's antiquated constitutional system, the paralysis of its parliamentary institutions, civil-military relations, and the alleged primacy of foreign policy. All of them also include helpful bibliographies on their subject matter. In addition, most of the papers do not content themselves with mere stocktaking, but make their own thoughtful contribution to their topics.

Only a few of these analyses can be touched upon here. Hans Boldt, probing the Empire's constitutional structure, points out the dependence of that system on some forceful agent of integration, given the lack of coordination of its component parts and the centrifugal momentum of conflicting social interests. Bismarck could generate some measure of integration by means of his immense prestige and his skillful use of the monarchy, but William II was no longer able to do so. William's failure to fulfill this unifying function caused the gradual disintegration of the government, a virtual stalemate in domestic affairs during the last prewar years, and the ascendancy of the military during the First World War. Michael Stürmer analyzes Bismarck's strategy of safeguarding the dominant position of a largely preindustrial elite by keeping alive the *Bürgertum's* fear of a social revolution. Hans-Jürgen Puhle calls attention to the dichotomy resulting from the growing ties between political parties and interest and pressure groups. While the labor unions had a restraining effect on the Social Democratic party, the association of the Conservatives and National Liberals with nationalist leagues and agrarian and industrial organizations made these parties more aggressive and uncompromising. Thus the net result of these extraparliamentary pressures was to impede parliamentary attempts at reforms due to the determined resistance of the conservative parties and the diminishing militancy of the Social Democrats.

Two papers, one by Helmut Böhme and the

other by Hans-Ulrich Wehler, deal with Bismarck's economic and foreign policies. From their different points of departure both authors point out the sharp contrast between the chancellor's economic and foreign policies toward Russia: this conflict culminated in his negotiation of the Reinsurance Treaty with St. Petersburg at the very time at which he was engaged in an economic war with the Russians. Here foreign policy could not claim primacy over domestic interests. A re-examination of the nonrenewal of the treaty after the dismissal of Bismarck is clearly called for in the light of these findings.

Among Wehler's collected essays a paper exploring the efforts to integrate Alsace-Lorraine into the Empire again points up the disparities between its social and political structure that helped doom all such efforts to failure. An analysis of the Zabern incident of 1913 provides important new data on the legal aspects of that event, based on materials Wehler found in the Berlin Hauptarchiv. Two essays on the trials of the Polish minority in Imperial Germany develop ominous parallels to later times, while a survey of the history of German-Polish relations deserves attention because of the conclusions Wehler draws from this melancholy story in regard to the present-day situation. In an essay on "Problems of Imperialism," Wehler, following up a main argument of his book *Bismarck und der Imperialismus* (1969), pays special attention to imperialist expansion as an effort to safeguard the political status quo of the mother country—a theme on which Stürmer also touches in the introductory essay to his symposium.

Most of the papers in both books are held together by several underlying theses, which inevitably makes for some repetitiveness. The Empire, it is stressed, was the creation not merely of blood and iron, but also of coal and steel. This combination of military, agrarian, and industrial forces also dominated the Empire's subsequent history. Its adverse effect manifested itself in the continuing conflict between a loosely organized preindustrial system of government and the new social forces that industrialization and modernization thrust to the fore. Most contributors are convinced that the resulting political stalemate could have been resolved only forcibly, even if there had been no war. All authors are, moreover, agreed that

there was a strong element of continuity between the Bismarckian and Wilhelmine phases of the Imperial era and that Bismarck's dismissal in 1890 did not constitute the deep incision in the Empire's history for which it has generally been taken. Indeed, the reader gains the impression from most of these essays that this history not only displayed a notable continuity, but was also marked by some sort of inexorable inevitability.

This impression, perhaps unintended, is reinforced by the fact that both books are focused on the deficiencies of the Empire's governmental system and the political and socioeconomic forces supporting it. There are of course incidental comments on the oppositional elements in several of these essays, but no attempt is made to explore systematically their potentialities and their weaknesses. Such an examination would have been helpful also because one of the contributors to Stürmer's volume, Gustav Schmidt, does suggest in an otherwise somewhat mystifying paper on "Germany on the Eve of the First World War," that the growing strength of the Socialists opened up possibilities of future reforms and that the constitutional system was flexible enough to adjust to such changes. This might also have provided an opportunity to examine the pertinent East German literature that is largely ignored except for a few asides.

These are minor reservations, however. Students of Imperial Germany will find both volumes stimulating and informative reading.

ANDREAS DORPALEN

Ohio State University

CHRISTOPH WEBER. *Kirchliche Politik zwischen Rom, Berlin und Trier 1876–1888: Die Beilegung des preussischen Kulturkampfes*. (Veröffentlichungen der Kommission für Zeitgeschichte bei der Katholischen Akademie in Bayern. Series B: Forschungen, Number 7.) Mainz: Matthias-Grünwald-Verlag. 1970. Pp. xix, 198.

Weber's book describes the differences of two church parties, the "intransigents" and the "liberals," and relates this "inner" conflict to the "outer" conflict of Church and state. The author concentrates his attention on the bishopric of Trier where he finds some of the leading figures of both parties.

Between intransigents and liberals lay an id-

eological chasm. The intransigents stood against enlightened influences and espoused an authoritarian hierarchy and an unsullied dogma. The liberals wanted the Church to accommodate to the new spirit of freedom. "Scholarship," wrote the cathedral provost Karl Holzer, "must move freely without tutelage." New influences, they felt, would strengthen, not weaken or corrupt the Church. Politically, the intransigents aligned with the Center and the Church liberals with the National Liberals or the Free Conservatives. The intransigents were antigovernmental and came to regard the imperial throne as an "ephemeral fly" compared to the venerable Church. The liberals tended to be friendly to the government and felt more comfortable living in the modern state.

Weber's most engaging proposition is that "the *Kulturkampf* must be regarded as part of the struggle of the two tendencies within the church." While stopping short of placing responsibility for the *Kulturkampf* on the Church liberals, he has discovered some remarkable statements in their correspondence revealing their sympathy for the objectives of the political antagonists of the Church. Thus, Holzer wrote that the real struggle was not with the Catholic Church but with the "Roman and jesuitical swindling," and Professor Kraus asserted that freedom for the Church too often meant freedom for pope and bishops to "exercise an illimitable despotism under the protection of a benevolent Government," a position very nearly coincidental with Bismarck's.

In the 1880s the opposing Church parties played their role at every turn in the complex maneuvers, which led to the amelioration of the *Kulturkampf*. The liberals tendered their good offices to Bismarck, welcomed the conciliatory policies of Leo XIII, and acted as his agents. The intransigents stood foursquare against conceding any of the claims made by the state. So adamant were they that even the pope felt obliged to move in secret lest he encounter their obstruction.

Weber concludes that stiff-necked, anti-Catholic elements in the Prussian bureaucracy as well as the ultramontane-intransigent elements in the Church not only delayed the dissolution of the *Kulturkampf* but prevented the realization of harmony and goodwill between Church

and state. That moderates in the government did not combine with liberal churchmen, Weber holds a misfortune for Germany.

This study rests upon impressive research, much of it in unpublished sources in the Episcopal and the City Archives in Trier and in the State Archive in Koblenz. Much of this material has not been heavily exploited, and Weber has brought to light documents of considerable interest. The book has the inherent weakness of all studies with a strong local focus, that is, its conclusions must be regarded as suggestive rather than definitive. Only a few figures, mostly churchmen in Trier, are considered in any detail. Obviously, before we can gauge the impact of the inner strife in the Church on Church-state relations, we must have similar studies of bishoprics elsewhere in Prussia and Germany. I hope that Weber or others will continue this promising work.

R. LOUGEE

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KENNETH D. BARKIN. *The Controversy over German Industrialization, 1890-1902*. Chicago: University of Chicago Press. 1970. Pp. x, 307. \$11.50.

This interesting but specialized study finds that the Prussian Junker insistence on government aid in the form of high tariffs on grain was a function of their failure to cope with the modern age and thus proof of their expendability as a ruling class. Barkin broadens the picture by also treating the support against industrialism given by such economists as Adolf Wagner, Karl Oldenberg, and Max Sering. Convinced that unfettered industrialism meant loss of ethical standards and social responsibility, they were determined to keep Germany moral, Christian, and rural. The book's narrow economic approach imposes economic values: industrialism is good; anti-industrialism is bad—an old-fashioned view for 1970. Barkin describes the conservative anti-industrial criticism but does not sympathize with it. With their large agricultural estates, the Junkers were themselves capitalists, yet they fiercely repudiated the role—why? Although Barkin recognizes the predominantly religious, ethical, and esthetic motivation of the conservative economists, he deals with the Junkers as if their motives were purely economic. Although

the book is full of tables on prices, cost of living, agrarian production, exports, and so forth, no data are provided on Junker holdings of stocks and bonds, their level of education, actual career choices, marriages, church attendance, theological orientation, for example. It is not strange that the Junkers tried to reap special rewards for their elite position; what ruling class has not? The problem is why they were not more successful. That they were placed so strongly on the defensive in 1902 demonstrates the surprising strength in Germany of the liberal, pro-Western modernizing forces. The Junkers' lack of flexibility probably resulted from their virtues rather than their greed—strong faith, loyalty to traditional values, and devotion to discipline. If they had been more corrupt, they could have become more modern. Barkin seems to think that a social revolution would have been helpful in promoting the cause of "sound economics," but this is doubtful, to say the least, the French, Russian, and Chinese examples not being very encouraging. From Taff Vale to Briand's drafting of railroad strikers, the European propertied and politically dominant classes seemed to become less flexible and more arrogant as they approached 1914. This book describes an important aspect of the phenomenon in Germany, but it does not explain it. One grows impatient for a study of the Junker class in depth, using sociological, cultural, and psychological as well as economic insights and methods. Nothing less will do.

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MARTIN SCHUMACHER, editor. *Erinnerungen und Dokumente von Joh. Victor Brecht, 1914 bis 1933*. (Quellen zur Geschichte des Parlamentarismus und der politischen Parteien. Third Series, *Die Weimarer Republik*, Number 1.) Düsseldorf: Droste Verlag. 1970. Pp. 425. DM 68.

Johannes Victor Brecht, whose political career is the subject of this valuable work, would have proudly agreed with the editor's description of him as a "conservative politician." For Brecht, the enemy was clearly on the Left. In a long and varied political career he boasted that his political principles stayed unchanged and that

Bismarck remained his political *Lehrmeister*. Brecht was not only a politician, who served in posts as varied as a Marburg city councilman and minister of justice under Brüning, but also a historian of note. His academic career is not stressed in the book, however, and his personal life is largely ignored.

The volume includes a sketch of his career and his political values by the editor, a description of the Brecht *Nachlass*, a list of his publications and speeches (378 publications ranging from scholarly book-length studies to newspaper articles and hundreds of speeches in the Prussian Landtag and the Reichstag), the last seven chapters of a ten-chapter autobiography, and eighty-four documents from the *Nachlass* to illustrate the highlights of his political life.

The most valuable portion of this work is Brecht's contribution to the history of the Wirtschaftspartei in which he was a prominent leader from 1921 until 1933. Little has been written about this important *Mittelstand* splinter party, which was perhaps the most blatant class party among all the *Bürger* parties. While Brecht had a long and personal interest in the "little men" whose economic interests the Wirtschaftspartei was formed to protect, and while he shared their conservatism and nationalism, he seems defensive about his association with this ragtag party. He claims his responsibility was to lead the party on the "great issues" of politics while Hermann Drewitz, a master baker, represented the special economic and tax concerns of the *Mittelstand* pressure groups that furnished the party's electorate. Brecht never clearly indicates why the traditional liberal and conservative parties failed to represent *Mittelstand* interests adequately or why the "little men" flocked to the Nazis after 1930. His personal attitude toward the rise of nazism was demonstrated in his opposition to Wilhelm Groener's tough stance toward the Nazis and the nazification of junior officers in the army. In this instance, while minister of justice, he seems to have aligned with those who wished to appease nazism.

Aside from the importance of this book as a biographical study and as party history, there are some fascinating anecdotes from the revolutionary period and several important behind-the-scenes glimpses of the Brüning government in its first nine months. To me, Brecht remains

a rather enigmatic figure yet representative of the political values of millions of German Bürger.

BRUCE B. FRYE

Colorado State University

WOLFGANG BENZ. *Süddeutschland in der Weimarer Republik: Ein Beitrag zur deutschen Innenpolitik 1918–1923*. (Beiträge zu einer historischen Strukturanalyse Bayerns im Industriezeitalter, Number 4.) Berlin: Duncker & Humblot. 1970. Pp. 371. DM 68.60.

Benz has produced an admirable study of the complex relations of the three South German states, with Hesse occasionally added, with each other and the Reich. An American correspondent years ago called the Main River Germany's Mason and Dixon line. So it has sometimes seemed, yet even in the apparently favorable circumstances from 1918 to 1923 nothing like a South German Confederation developed.

The author discusses the varied forms and results of the November revolution in the South and the efforts in 1918 and 1919 for a South German Commission. All that emerged from the common opposition to Berlin centralization was a series of *ad hoc* conferences, fairly frequent but not a united front.

Probably the chief reason for this failure was Bavaria's special particularism, nurtured as vigorously under the Independent Socialist Premier Kurt Eisner as under the conservative Gustav von Kahr. This particularism took a different form in the crisis of 1923 when extremists of the Right dreamed of a march on Berlin from Munich and Bavaria appeared to them, like East Prussia in 1813, the guardian of German nationalism.

Not that Württemberg and Baden always played a softer tune than Bavaria. For instance, on the thorny question of maintaining legations to other German states, Württemberg was sometimes more obstinate than Bavaria. But, by and large, a highly-blown federalism, emphasizing legal and traditional rights of the individual *Länder*, was more pronounced and more constant in Bavaria than in the other South German states. The conflict over the dissolution of the *Einwohnerwehren* is an example. Bavaria made this an overriding issue, while neither Baden nor Württemberg viewed

the continued existence of these organizations as a major concern.

Moreover, the Southwestern governments thought with good reason that the Bavarian lion roared too loudly (Benz's phrase) and that Bavaria's program was essentially for itself and not for South Germany. Hence their lack of enthusiasm for Kahr's attempt in 1920 to energize a South German front—his failure then was in Benz's view a turning point in the efforts for collaboration—and their ultimate willingness to go along with Bavaria only on some specific issues of states' rights.

Benz has covered the archival materials with great thoroughness and is familiar with the press, the pertinent memoirs, and the secondary literature. The only criticism is that the dissertation base shows through; the mass of detail, despite clear organization, at times blurs the main points, and one wishes for somewhat more synthesis, especially on social aspects and public opinion.

REGINALD H. PHELPS

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ERICH WITTENBERG. *Bismarcks politische Persönlichkeit im Bilde der Weimar-Republik: Eine ideengeschichtliche Beleuchtung einer politischen Tradition*. Volume 1, *Geschichte und Tradition von 1918–1933 im Bismarckbild der deutschen Weimar-Republik: Ideengeschichtliches zum Aufkommen des totalitären Staates*. Jericho, N. Y.: Paul A. Stroock. 1969. Pp. 319. \$8.50.

At a time when revolution is an advertising theme and future shock the subject of a best-selling book, Professor Erich Wittenberg addresses himself to the problem of continuity. No society, he declares, can function without tradition. Even ideologies such as Marxism or fascism, which deny tradition in principle, have in practice sought links with the past.

Why then is recent history characterized by repeated attempts to break with or ignore tradition? Wittenberg suggests the reason can be found in the rise of technology and the accompanying tendency to misapply its principles and thought patterns to spheres of human behavior. Technicians tend to be neophiles, adherents of an uncritical belief in progress. They find it difficult to understand the past, even when motivated to do so. The results can be illustrated, he feels, by a comparative study

of the images of Bismarck in the Weimar Republic.

The republic, Wittenberg argues, foundered in part because of its inability to come to terms with its heritage and to develop a living, coherent tradition. It came into existence at a time when technology, apparently triumphant in World War I, strengthened antihistorical tendencies in Germany. Yet at the same time the Weimar generation was driven to seek contact with its past. Motives varied from a search for principles of political conduct at a time of confusion, to a desire to understand defeat, to simple nostalgia. But the results were identical. Bismarck and his work took the center of the republican stage, and the resulting controversies illustrate the problem of relating to tradition in an antitraditional era.

In the present study Wittenberg offers little more than a theoretical framework. Final evaluation of his hypothesis must await the completion of two projected volumes analyzing in detail Bismarck's place in the intellectual life of Weimar Germany. However, his discussion of the chancellor's character and achievements is complete in this work. By no means an uncritical admirer of Bismarck, Wittenberg nevertheless takes his place with those scholars who see the decline of the Second Empire as beginning in 1890. He regards Bismarck as the greatest of European statesmen, the promoter of international peace and stability, the successful opponent of militarism, the creator of a viable *Rechtsstaat*. The portrait, in short, is unbalanced, tending to neglect or explain away the demonic, revolutionary elements of Bismarck and his work. One awaits with interest the effect of this concept on subsequent volumes.

DENNIS SHOWALTER
Colorado College

HANS-JOACHIM SCHOEPS. "*Bereit für Deutschland!*" *Der Patriotismus deutscher Juden und der Nationalsozialismus. Frühe Schriften 1930 bis 1939: Eine historische Dokumentation*. Berlin: Haude & Spenersche Verlagsbuchhandlung, 1970. Pp. 316. DM 28.

KARL A. SCHLEUNES. *The Twisted Road to Auschwitz: Nazi Policy toward German Jews, 1933-1939*. Urbana: University of Illinois Press, 1970. Pp. 280. \$7.95.

At the end of February 1933, a small organiza-

tion of about one hundred Jews was founded in Kassel, Germany, to proclaim its love for the fatherland in difficult times. Rejecting Zionism and rejected by Hitler's Reich, the organization adopted as its slogan *Bereit für Deutschland*—Ready for Germany. In its name *Der Deutsche Vortrupp, Gefolgschaft deutscher Juden* (German Vanguard, Followers of German Jews) it affirmed Germany twice, and its first pamphlet bore the title "We Walk a German Road." The leader of the *Vortrupp* was a man who called himself then and describes himself now as a "conservative, Prussian, and Jew" in that order. He is Hans-Joachim Schoeps, historian and professor, who in this book republished his essays of the 1930s about the Jewish predicament under the Nazis.

Schoeps left Germany for a wartime exile in Sweden and returned in 1946 to an academic life in the ruins. Instead of his young, conservative, Prussian Jews he now faced former Hitler youths in faded uniforms, home from a disillusioning war. As an old man, somewhat "old-fashioned," he still regards himself as a Jew, still ready for Germany. He knows that the Jews died—his book is dedicated to a *Vortrupp* comrade who was killed in the freezing experiments at Dachau—but he still lives with the motto that the *Vortrupp* adopted more than thirty-five years ago in its futile gesture to the German people: "Despite Everything."

Schoeps is a political curiosity, and possibly the most significant fact of his political life is that a German publisher would have reissued his writings in 1970. On the other side of the world, the young American historian Karl Schleunes is as far removed from the events of the 1930s as Schoeps was enmeshed in them. Schleunes made a sober attempt to understand the evolution of the anti-Jewish destruction process in its early stages.

Starting with the overwhelming fact of Auschwitz, Schleunes sees in the original measures of the Nazi regime a startling degree of disorganization. The pressure exerted against the Jews was not the product of a single policy, Nazi Germany was not a monolith, and Adolf Hitler did not resolve internal conflicts and did not issue decisive orders. These are the findings that constitute the main theme of *The Twisted Road*. To be sure, such insights are not new, and neither are most of the facts he

marshals in support of them. The book serves mainly to underscore the point for the benefit of those (and there are many) who have not yet grasped it. Occasionally Schleunes presents hitherto unknown material from Nazi party records as examples of the confusion and strain in the machinery of destruction. At the same time, however, he does not fully appreciate the steady advance of the civil service in its law-making functions, and he is particularly near-sighted when he declares that the "Nuremberg laws brought Nazi Jewish policy to the end of its legal phase." Still, *The Twisted Road* is a smoothly written work by someone who labored honestly to see for himself how things actually transpired.

RAUL HILBERG
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HANS-ADOLF JACOBSEN, editor. *Hans Steinacher, Bundesleiter des VDA 1933-1937: Erinnerungen und Dokumente*. (Schriften des Bundesarchivs, Number 19.) Boppard am Rhein: Harold Boldt Verlag. 1970. Pp. lxviii. 623.

This volume in the series of publications of the Bundesarchiv at Koblenz concerns the career of Hans Steinacher, who served as chairman of the Volksbund für das Deutschtum im Ausland (VDA) in the period 1933-37.

Steinacher was born May 22, 1892, at Bleiberg in Carinthia. After service in World War I as an officer in the old imperial army, he played a leading role in the struggle over Carinthia and the subsequent plebiscite. He participated in similar activities in Silesia and the Tirol, and in the Rhineland antiseparatist campaign. After a period as director of German library activities abroad Steinacher was chosen at the end of April 1933 as national leader (Reichsführer, from 1934 Bundesleiter) of the VDA.

The volume comprises a brief biographical sketch of Steinacher, a short treatment of the origins and growth of the VDA during the period of the Weimar Republic, and a series of 154 documents. These include contemporary texts bearing on the activities of the VDA during 1933-37 and memoranda composed by Steinacher during that period. There are also included, however, a number of other memoranda in the form of recollections written by Steinacher at various later periods of his life.

Jacobsen also benefited from a number of personal interviews with Steinacher. The combination of contemporary documents and later reflections has produced a certain unevenness in the points of view expressed by Steinacher and expounded by the author-editor.

Steinacher appears to have believed in 1933 that the VDA could be maintained as an independent organization without its being fully subject to Nazi control. This, of course, proved to be impossible. In his capacity as national leader of the VDA he was also exposed to the rivalries, jealousies, and internecine hostilities of those individuals and offices in party and state who aspired to control the policies affecting Germans outside the Reich. Steinacher seems to have been no match for this sort of competition. Before the end of 1937 he had been relieved of his duties with the VDA by Rudolf Hess, and the VDA was subordinated to the party. Steinacher himself was thereafter unable to secure any governmental or other post until, in the course of World War II, he resumed a military career, serving finally as post commander at Kirkenes, Norway. His later career, in the course of which he served until 1958 as Austrian consul general at Milan, is dealt with only incidentally in the present volume.

The interesting question of party membership in the leadership of the VDA is not fully covered. Steinacher in his postwar memorandum on his dismissal describes his interview with Rudolf Hess at the Brown House on October 24, 1937. In the course of it Hess detailed the charges against Steinacher, including one that he was not a National Socialist, to which Steinacher said he had no reply. Dr. Robert Ernst, the deputy national leader of the VDA who gave up his offices in the organization in 1935 following persistent disagreement with Dr. Steinacher over policy questions, is stated to have joined the party in 1933. Perhaps more definitive information on this subject could have been secured from the files of the Berlin Document Center.

The volume is carefully edited and presents a fascinating picture of an official attempting to identify his friends and enemies in the field of Nazi officialdom dealing with Germans abroad, and finally finding that enemies predominated, a situation shown graphically in

some of the organizational diagrams that are supplied.

JAMES S. BEDDIE
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ROLAND SARTI. *Fascism and the Industrial Leadership in Italy, 1919-1940: A Study in the Expansion of Private Power under Fascism*. Berkeley and Los Angeles: University of California Press, 1971. Pp. xii, 154. \$8.00.

The thesis of this excellent monograph is that, under the Fascist regime, the General Confederation of Italian Industry increased its power as a private pressure group and acquired new functions as a public agency. Many writers have commented on the collusion of "big business" with the Fascist leadership, especially at first, but Sarti maintains that the "big business" involved was the *Confindustria* itself, rather than individual firms and trade associations. Although his documentation and arguments are convincing, one still wonders how the *Confindustria* fared so much better than the League of German Employer Organizations, which was co-opted within three months after Hitler came to power. Sarti's answer is that the Fascist regime was not really as totalitarian as it claimed, that Mussolini was more of a mediator than is usually believed, and that the Italian industrial elite was particularly resourceful in its dealings with him. Sarti does not hide his pro-labor and Marxist sympathies, but his presentation is scrupulously balanced and fair to all parties.

According to Sarti, the industrialists gained the most in the long run from the March on Rome itself. The new regime soon created the authoritarian climate that the industrialists needed to run their factories without having to worry about interference from organized labor or liberal politicians. The labor reforms of 1925-26 entailed a rejection of the principle of *laissez faire*, to be sure, but they permanently removed the threat of strikes and gave the *Confindustria* more effective control over individual employers and greater influence over public administration. During the economic depression of the 1930s government restrictions on private initiative, especially with the policy of autarchy, helped the big producers to eliminate the last vestiges of competition and virtually monopolize the national market. The

industrialists faced setbacks too: the overvaluation of the lira in 1927, the consolidation of public enterprise in the mid-1930s, and the alliance with Germany, which partially freed Mussolini from his domestic supporters by giving him an outside base of power. But, as Sarti rightly points out, the debits to business, which were mostly in the form of fears that the radical Fascists might take over, were far exceeded by the credits, which were real and lasting.

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R. R. BETTS. *Essays in Czech History*. [London:] University of London, Athlone Press; distrib. by Oxford University Press, New York, 1969. Pp. xv, 315. \$11.25.

From 1946 until his death in 1961, R. R. Betts was Masaryk Professor of Central European History at the School of Slavonic and East European Studies at the University of London. A noted authority on the history of Bohemia in the Middle Ages, he left behind no book of his own. This work is the result of a posthumous effort by three of his former friends and colleagues (Peter Brock, G. H. Bolsover, and Otakar Odložilík) to collect and publish his major articles and lectures in a single volume.

Included are a brief memoir of Betts by G. R. Potter and fifteen essays that originally appeared between 1939 and 1957. They deal almost exclusively with Betts's special interest, Jan Hus and the Hussite Reformation, set in the broader European context of the fourteenth and fifteenth centuries. There is a core essay on Hus himself and a moving appreciation of Hus's brilliant, flamboyant fellow-martyr, Jerome of Prague. In two essays ("The Place of the Czech Reform Movement in the History of Europe" and "National and Heretical Religious Movements from the End of the Fourteenth to the Middle of the Fifteenth Century"), Betts develops the view that Hussitism was at once "part of a social, moral, and political revolution affecting the whole continent" (p. 87) and unique—"the first body of heretical thought to become identified with a successful experiment in national independence" (pp. 117-18). Elsewhere (p. 194) he restates the traditional Czech interpretation that with the Hussite Revolt "the Protestant schism

had begun, and the first great breach had been made in the ecclesiastical unity of western Christendom." Much attention is paid to the nominalist-realist dispute and its reflection in the theology of Hus and other Bohemian reformers ("The Influence of Realist Philosophy on Jan Hus and His Predecessors in Bohemia"). English-Bohemian ties and similarities are repeatedly brought up, especially the precise delineation of the influence on the Bohemians of Wyclif ("English and Czech Influences on the Hussite Movement"), the Lollard Peter Payne, and Richard FitzRalph, archbishop of Armagh and formulator of the doctrine of dominion founded on grace. There is some specific consideration of political, social, and economic factors in the chapters on "Some Political Ideas of the Early Czech Reformers" and "Social and Constitutional Development in the Hussite Period," the latter dealing with the depression of the peasantry and the growth of a *Ständestaat* in Bohemia in the fifteenth century. Finally there are two background pieces ("The Great Debate about Universals in the Universities of the Fourteenth Century" and "Society in Central and in Western Europe: Its Development towards the End of the Middle Ages") and three others only tenuously connected with the major themes—two on the foundation and early history of the University of Prague and one (perhaps the most original essay in the book) on the philosophy of history of Tomáš G. Masaryk.

Considering the paucity of writing on this subject in English, this collection has considerable value, despite various weaknesses. Betts had a thorough knowledge of the relevant source material, especially the contemporary literature and the extensive Czech bibliography, which he digested thoroughly and synthesized in these essays. To be sure the level and style of his exposition limit its appeal to readers who are themselves scholars, advanced students, or laymen who are unusually well versed in medieval history and civilization. Although the editors have thoughtfully transposed extensive Latin quotations to the footnotes and replaced them in the text with English translations, Latin phrases are still plentiful. Betts's writing style is rhetorical, magisterial, often ponderous, and antiquated (e.g., "galli-

maufry"). Thus assembled, the essays not only re-enforce but repeat one another extensively. The author's warmly pro-Czech, pro-Hussite sympathies and his anti-Catholic, anti-German biases are clearly evident. Finally, Betts's interpretation of the Hussite Reformation is seriously limited and dated by now. Although he describes it as "part of the general European middle-class revolution" (p. 91) and "fundamentally a social phenomenon" (p. 93), his treatment of social and economic factors is relatively brief and general. His concern is primarily with its religious, philosophical, and political-national aspects, and in this he essentially summarizes the conclusions of the mainstream of Czech historiography before World War II. There are only passing references to the newer Czech Marxist interpretation and, of course, no trace of the new psychological and sociological theories increasingly being applied to the study of mass religious phenomena. The editors have added newer titles to some of the author's bibliographical footnotes but have not gone any further in attempting to update the essays themselves.

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Istoriia Mist i Sil Ukraïnskoi RSR [History of the Towns and Villages of the Ukrainian SSR]. *Donets'ka Oblast'* [Donets Province]. Edited by P. O. PONOMAR'OV *et al.* (Holovna Redaktsiia Ukraïns'koï Radians'koï Entsyklopedii Akademii Nauk URSR.) Kiev: Instytut Istorii Akademii Nauk URSR. 1970. Pp. 991.

This latest volume of the monumental Ukrainian regional history resembles earlier items in the series in format and collective authorship. Donets *oblast* contains over one-tenth of the Ukrainian SSR population and an even larger proportion of large and medium-size cities. The present volume is, therefore, especially valuable for students of urban affairs with a historical as well as a contemporary focus. Since the territory was settled very late (essentially after Catherine II's elimination of the Crimean Tatar danger, in large part not until the mid-nineteenth century), the rich data on evolution of land tenure and rural communi-

ties that other volumes contain could hardly be expected. One might have hoped, however, for a little more material on rural regions than the present volume contains, such as details on growth of party membership. On the other hand, the late nineteenth-century industrial revolution, as reflected in the growth of the mining and manufacturing centers, is treated in great detail. Because socialist revolutionary forces were far more heavily represented in the Donbas than elsewhere in the Ukraine, this volume will be especially valuable to historians of early twentieth-century Russia.

Treatment of the Soviet period is detailed, although somewhat uneven. What I found most surprising was the light that a few passages throw on a very recent puzzle of Kremlinology. Students of post-Stalin USSR will recall that the abrupt dismissal in June of 1953 of Leonid G. Melnikov as Ukrainian party first secretary appeared to indicate a new peak of strength for Lavrentii Beria, whose associates sharply criticized Melnikov for his Russifying policy. Yet Beria's fall the same month did not mean rehabilitation for Melnikov, who was rarely if ever mentioned during the succeeding Khrushchev decade. The present volume not only praises Melnikov at several points, but states (p. 288) that he directed the underground organization after German occupation in 1941. While other passages hint that the 1941 resistance movement was not highly successful, the volume praises its efforts and suggests a continuity with the reorganized underground of the spring of 1942. During the Khrushchev period, on the other hand, a major unpublished dissertation (which I discussed in *The Soviet Bureaucratic Elite* [1959]) condemns the earlier resistance as a traitor-riddled failure. What now seems apparent is that this interpretation was designed to heap more criticism on Melnikov (probably a Malenkov associate), who could be rehabilitated only after Khrushchev's fall.

Use of this volume, like others in the series, for reconstruction of party imbroglios or for more conventional historical biography is greatly facilitated by the comprehensive name index. The reader whose Ukrainian is minimal should, however, be aware that initials may be misleading because Ukrainian versions of given names and patronymics may be quite different

from the more familiar Russian forms (for example, "Aleksandr" becomes "Oleksander," "Nikolai" becomes "Mykola").

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GERHARD SIMON. *Die Kirchen in Russland: Berichte; Dokumente.* [Munich:] Manz Verlag [1970.] Pp. 228. DM 18.

The new book by the young German historian Gerhard Simon concerning the position of churches in the Soviet Union is one of the best documented works on the subject. Other authors, such as Nikita Struve and Michael Bourdeaux, also master the subject adequately; Simon, however, has the advantage of viewing the history of the Christian churches in the Soviet Union in the light of the years that preceded the Russian Revolution. Another of the author's strong points is that he is very cautious in judging the religious leaders of the Soviet Union. He approaches a theme that cannot fail to provoke strong emotions in a strictly scholarly manner. It is precisely for this reason that the image drawn by his book is a most tragic one.

The two introductory chapters—the first devoted to the relations between the Church, the state, and society in prerevolutionary years, the second to the person and activities of Metropolitan Anthony (Vadkovski) of St. Petersburg—continue and develop the author's earlier book *K. P. Pobedonostsev and the Church Policy of the Holy Synod* (Göttingen, 1969).

The author's central conception is as follows: the Church, which was in a state of rebirth and which was to prove in the near future its ability to survive despite unprecedented persecutions, was confronted with a government that, even after issuing a decree on religious tolerance in 1905, considered all religious activities of its subjects purely from the viewpoint of their usefulness for the preservation of the regime. This explains why the tsarist government put the brakes on all attempts of the Orthodox Church toward self-government and independence from the state and continued to limit the activities of non-Ortho-

dox groups. The author points out with justification that the analogy between the Church policy of the tsarist and Soviet governments deserves separate treatment. Such a study, however, should include a more detailed analysis of the political and legal outlook of the Old Believers as well as of Protestant sects. The author is correct in stating that these groups did not pursue revolutionary goals, but in fact their preaching frequently was marked by anarchism, perceived in an eschatological perspective.

Only five pages are devoted to the relations between the Soviet government and the Church from 1917 to 1959. This is almost exclusively an enumeration of restrictive decrees. It is amazing that the author says nothing about the countless confessors of faith who were either executed or died in detention in the twenties and thirties. It is hard to believe that Simon accepts the official Soviet version, according to which clergy and laymen who perished in these years were punished exclusively for belonging to the former "ruling classes" or to the political opposition. The author himself shows conclusively that any act in defense of the faith could automatically be classified as "anti-Soviet activity." Legislative activity itself in the Soviet Union is an expression of dictatorship, for there is no separation of powers. The same tendency to take at face value the government's official accusations, accusations designed to create a semblance of legality, is exhibited by the author when he writes about the court trials of the Khrushchev period (p. 72). Fortunately, the documents that Simon presents at the end of his book and the authenticity of which he conclusively demonstrates bear witness to the true nature of Soviet law. Stressing the fact that Church leaders in the Soviet Union pay the price of unquestioned loyalty to the regime in order to ensure the physical survival of their institutions, Simon is in no rush to declare them as simply agents of the regime. A compromise in worldly matters is not identical with the betrayal of faith. This is illustrated by the fate of Metropolitan Nikolai (Yarushevich), as presented by the author. Despite the author's occasional gullibility, the book fully deserves an English edition.

CYRIL FOTIEV
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ISAAC DEUTSCHER. *Lenin's Childhood*. New York: Oxford University Press. 1970. Pp. vii, 67. \$4.95.

The mission of the late Isaac Deutscher may be likened to Milton's in *Paradise Lost*: to justify the ways of God (the dialectic) to man. A convinced Trotskyist-Marxist, Deutscher could not believe in a fundamentally evil cosmos, despite the obvious sin and suffering in the present world. In his works on Stalin, Trotsky, and post-Stalin Russia he sought to explain how Soviet Communism's fall from grace after 1917 was merely temporary and explicable within a Marxist world view. In an eloquent English prose style that might do credit to Milton, had he written on twentieth-century politics, the Polish-born Deutscher defended the sanctity and ultimate destiny of the Russian Revolution.

His long-planned life of Lenin no doubt would have restated this conviction with considerable force, as even this single chapter on Lenin's early years suggests. In it Deutscher lays down the moral foundations for Lenin's life and describes the dedication and discipline that were to make him a great leader. The moral basis of the revolution is detected not only in the condition of Russian society but in the peculiarities of the Ulyanov family. Deutscher persuasively describes the rise of Lenin's father from his lowly Kalmyk origins, his dedication to the cause of educational reform in the reign of Alexander II, his bitter disappointment in its sequel. The more harrowing story of Lenin's brother Alexander and his execution on the day when Lenin was writing his final examination in mathematics at the gymnasium is also effectively retold.

While Deutscher acknowledges the major role of Lenin's mother in the home, her personality and background are less developed. Isaac Deutscher died before the publication in the Soviet Union of a volume of Ulyanov family letters, including many by Maria Alexandrovna (*Perpiska sem'i Ulyanovykh, 1883-1917* [1969]). Apparently he did not think the possibility that her father was a baptized Jew sufficiently plausible to merit discussion, even though rumors among Soviet scholars in recent years support this contention.

Although there are masses of evidence on Lenin's life, he remains an exceedingly reserved, even impenetrable, personality. Deutscher

was well aware of this problem and in this chapter on Lenin's early years could find no new access to the man's inner life. How he might have tried to write a biography that would go much beyond the political, we will never know.

It is appropriate that Deutscher's publisher should have paid tribute to his work by issuing this fragment from his last project, but what is one to say of the price that they assigned to so slender a volume?

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V. F. BULGAKOV. *The Last Year of Leo Tolstoy*. Translated from the Russian by ANN DUNNIGAN. With an introduction by GEORGE STEINER. New York: Dial Press. 1971. Pp. xxv, 235. \$7.95.

In January 1910, V. F. Bulgakov, a student of philosophy at the University of Moscow and a devoted Tolstoyan, became Tolstoy's personal secretary, replacing N. N. Gusev, who had been arrested six months earlier. Through the ensuing year, he was with Tolstoy constantly, answering his voluminous mail, compiling materials for his various didactic publications, and in those anguished last months acting as intermediary in the bitter dispute that finally drove Tolstoy from Yasnaya Polyana and to his death. *The Last Year of Leo Tolstoy* is a translation of about half of a diary Bulgakov kept during this year and later published. One naturally wonders why the entire Russian publication was not translated, but half a translation is better than none, especially when it is done as skillfully and smoothly as Miss Dunnigan's. A sensitive opening essay by George Steiner sets the work in context, traces the moral struggle of the final decades back through the earlier years, and briefly depicts Tolstoy's artistry.

Bulgakov's Tolstoy is, in the main, the Tolstoy of the standard biographies, since the diary has long been a principal source for them. We meet again the spartan champion of simplicity, love, and work, weeping easily and deeply concerned about everyone's troubles; the resolute enemy of property, conventional education, lawyers, doctors, human carnivores, high-society pleasures, modern literature, and the claims of the flesh; the robust aristocrat racing

his horse Délire along the most difficult trails, relishing a good joke, a song, a frenzied *hopak*; and the helpless victim of the intrigues, jealousies, spying, and suicide threats provoked by the fierce battle between his wife, Sofya, and his most militant disciple, Chertkov, over possession of his diaries and management of his literary estate.

There are, however, fresh insights. For one thing, one sees how extraordinarily busy Tolstoy was. Yearning only for peace and solitude, he was submerged in a sea of visitors, beggars, letters, books, compilations, articles, introductions, family gatherings, conferences with his assistants, the routine chores he insisted on doing himself, and his therapeutic and ritualistic work and exercise. And eighty-two years old! For those who tend to oversimplify his views, there are also some useful lessons. Fiery jeremiads against esthetes and sophisticates notwithstanding, the poetry he quotes is Pushkin, Fet, and Tyuchev; music is his great passion; and Haydn is his favorite composer. For all his attacks on industrialism and political violence, he has nothing against agricultural machinery, dissuades a young man from giving up work on the railroads, attends the auto races, aids a young revolutionary to escape from the country, is entirely sympathetic with Jews demonstrating against the state that oppresses them, and repeatedly praises the recent Portuguese revolution. As for his religious philosophy, finally, there is little, but enough, considering what Tolstoy says of it here. God, the Kingdom within you, is love, the force drawing man to man and to nature. Why ask for more, he wonders. Why pose problems that are beyond solution? Why all the mysticism and the miracles? The Resurrection is "superstition": Easter is just another day. It would be better to follow the Buddhists and Confucianists and do without "the concepts of God the Creator, of paradise, of a future life of bliss." What was the essential difference between Christianity and Buddhism, one visitor asked him. "True Christianity?" "Yes." "None. As in one so in the other, a God of love is propounded and a personal God denied."

ARTHUR P. MENDEL
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SHEILA FITZPATRICK. *The Commissariat of Enlightenment: Soviet Organization of Education and the Arts under Lunacharsky, October 1917-1921*. (Soviet and East European Studies.) New York: Cambridge University Press. 1970. Pp. xxii, 380. \$13.50.

Western writers investigating the early years of the Soviet regime quite naturally have concentrated on the problems that most concerned the Bolshevik leadership—the achievement of victory in the civil war, the economic reorganization of the country, the centralization of party control, the resolution of the nationality question—and have generally paid little attention to educational and cultural policies. Sheila Fitzpatrick's study on the Commissariat of Public Enlightenment (Narkompros) during the years of war communism and the first year of NEP should help correct this imbalance. Relying heavily on Soviet archives and the contemporary periodical press, she has exhaustively described the institutional structure of Narkompros as it developed from 1917 to 1921. At the same time she has given us a most thorough account of the commissariat's, and especially Lunarcharsky's, attempts to establish a more viable and progressive educational system in Russia and to pursue a policy of toleration and indeed encouragement in the arts. Miss Fitzpatrick's conclusions are most sympathetic to the commissariat. Although she admits Narkompros's failure in education, she argues that Narkompros should not bear the blame for this. Party and government indifference, the "inertia of institutions and above all . . . the poverty of the Soviet Republic" (p. 287) were the major culprits. On the other hand, the commissariat in her view deserves much of the credit for protecting the arts and sciences from excessive state interference. In fairness, however, one should point out that the party's preoccupation with urgent military, economic, and political matters during the chaotic days of the civil war must have prevented it from vigorously interfering in the arts.

Structurally, the book is sometimes disappointing. It is rather diffuse partly because of the very nature of the subject matter, but also partly because of the author's tendency to describe events rather than analyze them. The result is a monograph weighed down by much undigested material. Why, for example, does

the reader have to go through so many abortive plans on the organization of the commissariat particularly since, as the author seems to imply, Narkompros's organization had little effect on its accomplishments? True, the author's statements that she is writing "institutional history" and that the early history of the commissariat can provide a case study in the problems of revolutionary government appear to offer some justification for this excessive detail, but unfortunately she does not often adopt the methodological tools of institutional history or the case study approach to give her material more form. These weaknesses notwithstanding, historians should be grateful to Miss Fitzpatrick both for putting together this coherent narrative from not readily available sources and for her very useful appendix containing well over one hundred brief biographies of people involved in commissariat affairs. One hopes that her projected second volume on the commissariat will combine equally extensive research with a more analytical approach.

ALLEN SINEL

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I. E. VOROZHEIKIN *et al.*, editors. *Sovetskaia intelligentsiia (Istoriia formirovaniia i rosta 1917-1965 gg.)* [The Soviet Intelligentsia: A History of Its Formation and Growth, 1917-1965]. (Akademiia Obshchestvennykh Nauk pri TsK KPSS, Kafedra Istorii Sovetskogo Obshchestva.) Moscow: Izdatel'stvo "Mysl'." 1968. Pp. 431.

The most interesting feature of this history of the Soviet intelligentsia is the brazen inconsistency of the introductory definition. The great bulk of the intelligentsia is defined from a Marxist viewpoint as "an inter-class stratum," which turns out to be an occupational classification. They are *rabotniki umstvennogo truda*, professional and white-collar workers, and their outlook is determined by those who employ them. But a very select few, the genuinely "socialist intelligentsia"—in plain English, the Bolshevik leadership—is defined from a nationalist point of view. Their social role is determined by their outlook; in their case consciousness determines being. Since they alone could think correctly about the progress of history, they became the only true leaders of the toiling people, and "took on themselves the enormous burden of organizing the government of the

first socialist republic" (p. 10). (Elsewhere the reader will find the usual formula about workers and peasants ruling.)

The most useful feature of this book is the statistical information it contains on the development of the professional and white-collar labor force. It is not a great deal of information—nothing like the mountains of data in DeWitt's *Education and Professional Employment in the USSR* (1961) or the OECD's *Science Policy in the USSR* (1969)—and it will be very hard to use, for the book has no index. But it is there to be dug out, along with anecdotal tidbits from the archives. My favorite is the quote from a *rydvizhenets* (a worker or peasant "pushed up" to a position of authority without the usual qualifications) telling how he got information from uncooperative specialists by hitting them in the face (p. 140).

The most appalling feature of this book is its deliberate ignoring of inconvenient facts. For example, the chapter on the intelligentsia in the thirties has not one word to say about the terror that killed and jailed them in still uncounted numbers. Even the little litany about "mistakes" and "unjustified repression," which characterized Soviet histories in the Khrushchev era, has disappeared. Unfortunately this is no isolated case. For the past five years any effort to take a frank look at the Stalinist heritage has been put down by the "sharp struggle against attempts at ideological subversion, which are constantly undertaken by agents of imperialist reaction" (p. 429).

At least one of the contributors to the present volume, S. A. Fediukin, has in the past shown some readiness for de-Stalinization. (See his *Sovetskaia vlast' i burzhuaiznye spetsialisty* [1965], which he refrains from citing here.) We can only hope that his faltering spirit will some day be revived. *Ewig kanns nicht Winter sein*, as the peat-bog soldiers once sang.

DAVID JORAVSKY

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PETER KENEZ. *Civil War in South Russia, 1918: The First Year of the Volunteer Army*. Berkeley and Los Angeles: University of California Press. 1971. Pp. 351. \$10.00.

This work is essentially a study of the Volunteer Army and its Cossack allies during 1918. The development of this anti-Bolshevik force is

traced from early December 1917, when the five leading figures of the future army—L. G. Kornilov, A. I. Denikin, A. S. Lukomski, I. P. Romanovski, and S. L. Markov—made their way from imprisonment near Mogilev to Novocherkassk in the Don territory. The account ends in November–December 1918, after the close of World War I. It is unfortunate that the time period is thus abbreviated, for the tale of the first year of this most important White Army organization presents but a small part of a tragic, heroic, and pitiful military operation that cannot be fully understood unless it is carried up to the last months of 1920. In addition the too narrow concentration of the author upon the affairs of the Volunteer Army tends to isolate the army from the vast surrounding sea of national politics and fighting that made its existence significant.

While the book's main concern is to describe the Volunteer Army's military campaigns and less than totally successful efforts to gain cooperation from Cossack troops of the Kuban and Don territories, its most valuable contribution is provided by penetrating insights into the character of the military commanders and the quality of their political action. These men were Russian generals who believed that they were loyally struggling to preserve the nation and its honor. They were also, as Professor Kenez demonstrates, politically illiterate, so ignorant of political affairs and the normal aspirations of mankind that they moved through the catastrophe of civil war absurdly unaware of the issues for which men around them were dying. Valiant officers who had never been called upon to think about the world outside the army, full of army ethics and military concerns, they did not even suspect how abysmally they failed to understand what was happening to Russia and to themselves. In their eyes the politicians were fools, the agonies of the society around them were to be ignored, and martial brutality and unthinking patriotism were the supreme virtues. The weaknesses of the White side in the civil war are shockingly demonstrated by this story of lost and apolitical "leaders," marching from one Cossack village to the next in search of death.

This work, in general a solid and worthwhile study because of its careful research and exact presentation of details about men and military

actions, is marred by incautious generalizations about the causes of events and the motives of the actors. In addition, although Professor Kenez provides detailed descriptions of marches and campaigns, the absence of adequate maps (there is but one in the book) makes the actions described difficult to follow. A particularly disturbing weakness for me is the failure to examine the Bolshevik armies more carefully and sympathetically—with the result that the very human and suffering faces of the Cossacks and peasants on the other side of the battle lines are never clearly seen.

ARTHUR E. ADAMS

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NANCY WHITTIER HEER. *Politics and History in the Soviet Union*. Cambridge, Mass.: MIT Press. 1971. Pp. viii, 319. \$12.50.

Dr. Nancy W. Heer, an assistant professor of government at Wheaton College, set out to produce a functional analysis of historiography in the post-Stalin Soviet system, especially the historiography of the party. She reasoned that such a study, despite certain inherent limitations of the approach, might make more vivid the features of the larger system, historiography being in essence a subsystem of the whole. Her systematic, perceptive, careful, and scholarly study not only bears out the soundness of her reasoning, but also demonstrates a keener appreciation of the nature, uses, and values of history than can be found in the works of most historicists and many historians.

A brief introduction on the Marxist-Leninist theories of history with stress upon the ambivalence of these views (an "attraction-repulsion syndrome," as she labels it in one place) sets the backdrop for detailed narrative analyses. These are set forth under four headings: history (1) as a political subsystem, (2) as a political record, (3) as "the mirror of conscience," and (4) "as portent." Her closely dovetailed account of complex details precludes accurate summations within the space allotted this review; only general impressions and conclusions can be stated.

Dr. Heer's research has clearly been extensive as well as intensive. Although the book concentrates on the period from 1956 to 1966, well-based allusions to earlier developments

and their meanings are frequent and very helpful in adding perspective. She is well aware that her book is not encyclopedic and appears disturbed at having had to be rigidly selective. Her selectivity is, however, a virtue rather than a fault. Her criteria appear to have been sound and her judgments carefully considered. She puts forward her own views without dogmatism but with modest and merited assurance. She is familiar with Western studies relevant to her subject, none of which exactly match her own approach; but her main dependence is on published Soviet materials, which she has been most assiduous in finding and using. Her use of both official and "underground" (her label) reports of discussions and decisions at professional meetings—the historians' conferences of 1962 and 1965 being cases in point—is especially commendable.

Although Mrs. Heer is, broadly speaking, engaged in analyzing institutions, she never forgets that institutions are made up of people. Histories are written by persons, not by institutions though the latter provide guidance and constitute constraints upon the individual. Perhaps the most significant finding by Dr. Heer (and one she emphasizes very strongly) is that it is no longer correct to regard all Soviet historians, even party historians, as slave mechanisms of the system and its rulers. When the Soviet rulers began to demand and to provide better training for historians and higher standards of historical scholarship and when the rulers began to permit their historians to see and use hitherto arcane archives and other data, they set in motion aspirations, ambitions, and hopes that increasingly challenged official values.

The returns are not yet in and will not be for some time. General and specific restraints and constraints remain and so do honest differences of opinions and values. There is, as Mrs. Heer notes, both an ambivalence among officials toward history, historiography, and historians and also a quasi-schizophrenia among many Soviet historians. While recognizing that these will not vanish immediately—indeed, cannot do so as long as the bosses of the CPSU maintain their claim to monopolize truth and wisdom—Professor Heer concludes by hazarding the guess that the new history will erode the myth of party infallibility and so eventu-

ally open the way to "alternatives in the spirit of responsible citizenship."

A final note of advisement, not of complaint: *Politics and History* is not easy reading. This is due primarily to the nature of the subject, but the closeness of the book to the dissertation from which it stemmed is a contributing factor.

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NEAR EAST

MAJID FAKHRY. *A History of Islamic Philosophy*. (Studies in Oriental Culture, Number 5.) New York: Columbia University Press. 1970. Pp. xv, 427. \$15.00.

To undertake the writing of a history of philosophy requires, among other things, considerable courage since it is unlikely that either the historian or the philosopher will be altogether happy with the results. Philosophy does not easily suffer itself to be reduced to grist for the archeologist of thought; ideas make only indifferent potsherds. The pretense of the philosophical analyst, on the other hand, that the only correct approach to a philosopher is the taking up of the naked text and so confronting the thinker "on his own terms" is to deny, in the face of all the evidence, the existence of a philosophical tradition. Farabi and Ibn Sina made no attempt at concealing their immediate association with Plato and Aristotle; indeed, they insisted upon it.

Majid Fakhry, professor of philosophy at the American University of Beirut, is neither a historicist of ideas nor a mock-innocent analyst. There is some of both approaches in his *History of Islamic Philosophy*, but what Fakhry does most often, and best, is simply to narrate, to lay out what each major Islamic thinker said in his major writings. This might appear to be a modest enough enterprise, but it is not often done.

There is, of course, a price to be paid. As part of his admirably straightforward approach Fakhry eschews all special pleading, a dispassionate quality that is achieved only by avoiding both theme and issues. There once was a kind of *communis opinio* about Islamic philosophy, but it was an attitude born of ignorance, not of agreement. With more careful study has

come more sophisticated understanding and, inevitably, more reasoned disagreement. In refusing in the main either to argue or to plead, as Henri Corbin so outrageously and usefully does, for example, Fakhry has given a tenable version of the course of philosophy in Islam, but has failed, I think, to reveal to the reader what is now thought about that discipline by an increasing number of his contemporaries.

What the Islamic philosophers and theologians said and precisely what they meant by it is currently being studied more vigorously than at any time since modern scholarship turned its attention to the subject. Merely within the last ten years an entire generation of students has rewritten and expanded much of what was known about the field before 1960. This extraordinary academic quickening has deep foundations in the work of scholars and editors in the forties and fifties who opened the Eastern manuscript collections and taught Islamists to read the newly found texts in their full philological and historical context. Their instruction has borne rich fruit indeed. The major contributions of Laoust, Makdisi, Frank, Van Ess, Allard, and McCarthy to theology; of Stern, Daiber, and Endress to the translation literature; of Ivry to Kindi and Mahdi to Farabi; of Madelung to Shi'ism and Nasr to Iranian thought; of Hourani to Muslim ethics; and of Fuat Sezgin to the whole range of Islamic intellectuals are all less than ten years old.

It is clear even now that within another decade the history of Islamic philosophy, and particularly of theology, will have to be rewritten with results almost unrecognizable to anyone who had studied those subjects before 1960. In that complex and technical sense Fakhry's book is already out of date, as a glance at his bibliography will quickly reveal; there is little or no reflection there of the profoundly important researches of the scholars mentioned above.

On another level, however, it was necessary that Fakhry's book be written, and in precisely the manner it was. It has been exactly seventy years since this kind of unadorned historical narrative has been produced on the subject of Islamic philosophy, and the major difference between Fakhry's book and De Boer's *History of Philosophy in Islam* (1901; tr. 1903) is the

simple fact that many more original texts have been published in the interval, and Fakhry tells us what is in them.

There are other differences, to be sure. There is an extended introduction (pp. 12–50) to “The Legacy of Greece, Alexandria, and the Orient,” and Fakhry devotes considerable attention to areas unknown or ignored in De Boer’s day: the earliest tradition in dialectical theology (*kalam*) (pp. 51–81); mystical and ascetical theology (pp. 262–86); an enlarged place for the Christian theologians writing in Arabic (though none for their Jewish counterparts); gnostic speculation among the Shi’ites (still seriously undervalued); and the development of Islamic thought after 1300 (pp. 347–400). These are important additions, and they accurately reflect for the contemporary student the richness of the speculative tradition in Islam.

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G. E. VON GRUNEBaum. *Classical Islam: A History 600–1258*. Translated by KATHERINE WATSON. Chicago: Aldine Publishing Company. 1970. Pp. 243. \$6.00.

Writing a history of classical Islam is always a laborious and thankless task. The historian who undertakes to reconstruct in a narrative way any portion of early Islamic history from its vast and scattered sources exposes himself to sure attacks. The main sources of Islamic history consist of compilations of orally transmitted reports, whose accuracy could not be checked by the compilers because the eyewitnesses to the reports were no longer living at the time of compilation. A modern historian therefore has to use what R. G. Collingwood calls the “scissors-and-paste” historical method, by which the historian himself must make a personal judgment as to which reports are to be taken as true accounts of history and which are to be considered false. On both counts, such a historian would expose himself to criticism by his colleagues because no two historians use the “scissors-and-paste” method exactly alike.

In his introduction to the book under review, Professor von Grunebaum makes the following statement: “Some of the facts, observations, and ideas presented in this book may

seem to the specialist to be in need of documentary corroboration. The author agrees but has decided for once to expose himself to scepticism in the interest of an unbroken narrative and to keep the evidence in his desk whence he shall be glad to spread it out if the critics become too restive” (p. 8).

Let us suppose that the critics became too restive and requested documentary corroboration. Because of the nature of the sources of Islamic history, even if the author were to produce the evidence from his desk, critics would not be silenced; they would argue in terms of the validity, or lack of it, of whatever reported evidence is produced. For this reason I will not try to criticize the materials of this book from the point of view of why the author emphasized certain features of reported Islamic history and bypassed others; the author is certainly free to use the “scissors-and-paste” as he pleases. The evaluation of this book, however, will be limited to an assessment of whether or not it achieves the goals the author himself has set for it.

The chief goal of writing this book seems to be summarized in the following statement from the introduction: “For the moment, however, he [the author] has adopted the immediate interests of a hypothetical readership, scholar and lay, and the student in between, which he feels is primarily [interested] in receiving order in chaos rather than laborious arguments for one particular order in preference to another” (p. 7). From this statement, it appears that a major aim of this book is to acquaint the lay reader as well as the student with the history of classical Islam from 600 A.D. until 1258 A.D., without belaboring either with needless arguments. On closer look, however, one finds that the various chapters of this book contain selected highlights carefully discussed by the author. These highlights of classical Islam as they are presented cannot be readily grasped by either the lay reader or the student without prior knowledge of the details of Islamic history from other works. For example, the chapter on “Egypt Under the Fāṭimids and Ṭulūnids” is treated in only five pages and the important chapter on “The Horizon of Islam: Theology, Philosophy, Literature” in only thirteen pages—including two maps. Since Professor von Grunebaum is not known for noncreative

writing, the subject matter of those chapters as well as most of the other chapters is far from being a historical narrative. As a matter of fact, the book as a whole deals with discussions of certain problems in classical Islam as seen and analyzed by the author himself. As for the reaction of the scholarly readers, these discussions may or may not appeal to some specialists in the field of Islamic studies, whose ideas are already set and are beyond the beginner's level. Accordingly, among the possible readers of this book, only the advanced students specializing in the field of Islamic history are in a good position to benefit from these discussions.

Although the author tried to avoid "laborious arguments," I could recognize some unavoidable "laborious arguments" dealing with certain aspects of classical Islamic history. As an illustration, one may cite the argument concluding that Muḥammad was "a creative religious spirit and an emissary of God" (p. 39). The question is whether a historian can really arrive at such a conclusion regarding any religious leader.

Be this as it may, in this book, translated from German by a capable translator, the thoughts of the author are well-expressed. This makes the book very stimulating and enjoyable to read.

WILSON B. BISHAI
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STANFORD J. SHAW. *Between Old and New: The Ottoman Empire under Sultan Selim III, 1789-1807*. (Harvard Middle Eastern Studies 15.) Cambridge, Mass.: Harvard University Press. 1971. Pp. xiii, 535. \$15.00.

Although many histories of the Ottomans have been published, there are few studies of important sultans. In Shaw's examination of Selim III's reign, 1789-1807, we have not only a new interpretation of a pivotal figure in Ottoman history but also a work in English that makes use of the Turkish archives.

The central theme of this history is the failure of Selim III to prevent the disintegration of the Empire between 1789 and 1807. Beginning with a description of Selim's education, Shaw then introduces the reader to the dangerous international situation the sultan inherited when he assumed the leadership of the Empire in 1789. The details of Selim's reform program

subsequently show how the sultan reacted to the challenges of his time. But the depth and direction of the attempt to reinvigorate old institutions did not stop the process of disintegration. A provincial analysis of Ottoman politics reveals the near anarchical state of the Empire between 1792 and 1807. Moreover, Selim's inability to command the Ottomans led to the triumph of reactionaries, the dispersement of reformers, and his own death in 1808. Shaw concludes, however, that this dismal record was not entirely a negative experience for the Ottomans. Rather, the failures of Selim III taught the ruling class how necessary it was for them to replace old institutions with new structures.

Between Old and New follows H. A. R. Gibb's and Harold Bowen's study of Islamic society in the eighteenth century. Correcting many of the errors that crept into that pioneer work, Shaw strengthens the conclusions reached by Gibb and Bowen about the general disarray of the Empire in the last century of decline.

There is, however, opposition to this picture of the eighteenth century. Even though the Ottoman government had suffered decentralization by the time of Selim's reign, this did not mean that the Empire lacked internal cohesion. As Shaw pointed out, Ottoman history does not possess a developed literature on politics; but the problem where the eighteenth century is concerned is whether or not political history affords the best means of understanding a century in which social stability, religious tradition, and local custom were of supreme importance.

Whatever the outcome of the arguments between those who treat the eighteenth century as a unit and those who see the same span of time as the last act in a long history of decline, Shaw's study of Selim III represents a major step forward in Ottoman studies.

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EDGAR O'BALLANCE. *The War in the Yemen*. Hamden, Conn.: Archon Books. 1971. Pp. 218. \$7.00.

Those who relish rereading yesterday's newspapers might enjoy Major O'Ballance's latest effort. The author, a prolific publisher on military subjects, has specialized in writing books

on the civil wars and guerrilla conflicts that have convulsed a host of Afro-Asian countries during the post-World War II era. Many of O'Ballance's works tend to follow a formula: a historical introduction to set the stage, which includes pertinent political, economic, social, and cultural information; a detailed summary of the course of the struggle; a brief epilogue to bring the reader up to date on post-hostilities developments.

The War in the Yemen conforms to the model closely. Essentially, we are provided with a clear, if uninspired, chronology of the progress of the now quiescent civil war that vexed Yemen in the wake of the antiroyalist revolution of 1962. This is augmented by two unpretentious maps, ten prechapter chronologies, a good index, and Major O'Ballance's analyses of the strategy and tactics employed during the struggle. Among the most interesting parts of the volume are the sections that detail foreign—especially Egyptian—involvement in the fighting. Thus, the book is a convenient record of the military aspects of Yemen's recent ordeal.

But O'Ballance is not a specialist in Middle Eastern history and the factual errors and other lapses that mar his book betray inadequacies in his background knowledge. Many of his judgments—even his selections of illustrative material—are distorted by his Eurocentric values. His attitudes toward Yemen's tribal society and its premodern institutions are condescending. He tries but he fails to provide either a sound explanation of Yemen's culture and its values or a convincing interpretation of the causes of the 1962 rebellion. Such attempts as are made to place the Yemeni upheaval into the larger context of modern Middle Eastern history do not inspire much confidence. So the book stands basically as a rather narrowly conceived journalistic production. A truly comprehensive history of "the war in the Yemen" is yet to appear.

ROBERT G. LANDEN

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AFRICA

HUBERT DESCHAMPS, editor. *Histoire générale de l'Afrique noire, de Madagascar et des archipels*. Volume 1, *Des origines à 1800*. Paris: Presses Universitaires de France. 1970. Pp. 576.

Hubert Deschamps, professor of African history at the Sorbonne, has assembled a well-qualified group of nineteen scholars to contribute to the first volume of his history of black Africa. The first part, a massive tome of 572 pages, deals with the continent from the origins of man to A.D. 1800. We are promised a sequel on the nineteenth and twentieth centuries, which, like the first, will have three sections.

Each part of the *Histoire générale* is largely self-contained, and the first volume begins with a general introduction to black Africa. Gilles Sautter writes of the geography and environment of Africa, while in eleven breathless pages Raymond Mauny summarizes African prehistory, using the term in the French sense to refer to the period before literate history. We pass from his review of the Australopithecines and prehistoric stone technologies to a summary of the biological diversity of Africa by Jean Hiernaux; the first part ends with essays on languages, black African society, material culture, and historiography.

The history of black Africa from the origins of ironworking up to 1800 forms the major part of the first volume. Thirteen scholars have contributed articles on their particular specialties. Raymond Mauny gives us summaries of the western Sudan and of early voyages. Leclant deals with northeastern Africa, while West Africa receives the attention of Yves Person, Jacques Lombard, and other French authorities. Jan Vansina and Roland Oliver contribute able summaries of the southern savannah and East Africa respectively, while W. G. N. Randles summarizes the Zambezian area.

The final section of the book is devoted to three essays on the Indian Ocean and Madagascar, a welcome and unusual feature of this history. All too often Madagascar is omitted from histories of black Africa when in fact its contribution was both lasting and important, even if no Malagasche artifacts have yet been discovered on the mainland. August Toussaut and Hubert Deschamps describe the history both of the Big Island and of other archipelagos.

Textbooks on African history have been proliferating at a rapid rate in recent years. This development is a reflection of burgeoning enrollments both in African studies and Afro-American courses. Clearly Deschamps's volume

is designed for use by French undergraduates. All the basic facts on African history and some of the major theories are here, and there is little point in picking on details that are inaccurate or on sins of omission. Academically the biggest sin is that of out-of-date information. Raymond Mauny's prehistory chapter makes no reference to the exciting finds of *Australopithecus* in Ethiopia or the Lake Rudolph area of Kenya and is preoccupied with dates and stone technologies. Randles makes no mention of the important new Early Iron Age discoveries from 1966 onward in Southern Africa, nor does he discuss Peter Garlake's new chronologies for the Rhodesian ruins. One suspects that much of this book was written some years before it went to press.

Pedagogically speaking, I question the advisability of teaching the subject and laying out this book on the basis of territorial and geographical considerations. Maybe, as an archaeologist, I feel an attachment to chronology, which is reflected in a need to place peoples, cultures, and historical events in a chronological sequence. The complexities of African history and the sheer size of the continent make it difficult for a student to envisage the relationships of historical events in different parts of Africa without a chronological framework to work with. This volume would have been a more effective educational tool had the contributors been invited to write chapters about major historical problems, such as the trans-Saharan trade, the evolution of West African states, the early colonization of Madagascar and the east coast, and the emergence of Bantu Africa. A student to whom I showed this book commented that he had to sift through several chapters to obtain a summary review of the Greenberg-Guthrie controversy. The geographical divisions have the advantage that one can easily refer to the book for a summary of up-to-date literature about the history of different African countries. Much information of value about Francophone Africa can be obtained in this way. Deschamps's volume is likely to be of more use in American contexts as a basic reference source than as a textbook.

Despite the uneven quality of some of the contributions, this volume will be useful to many advanced students of African history. It is attractively produced, and the photographs,

predominantly with an ethnographic emphasis and distribution in Francophone Africa, are generally of high quality. What a pity Hubert Deschamps did not undertake to write this history himself with the aid of one or two collaborators instead of a multiplicity of authors. The result would have been more authoritative, easier to use, and probably of very high quality indeed.

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ROBERT I. ROTBERG and ALI A. MAZRUI, editors. *Protest and Power in Black Africa*. (Written under the auspices of the Center for International Affairs, Harvard University.) New York: Oxford University Press. 1970. Pp. xxx, 1274. \$25.00.

ROBERT I. ROTBERG, editor. *Rebellion in Black Africa*. New York: Oxford University Press. 1971. Pp. xviii, 261. \$2.95.

In their lengthy volume of readings on protest and discontent in black Africa, Professors Rotberg and Mazrui have attempted to bring together selections that illustrate a wide variety of causes and focuses of dissent at different periods of African history since the modern European conquest of the continent. The second title is a substantially reduced version in paperback of the main volume with a new introduction by Professor Rotberg.

To provide some measure of coherence to such a vast topic, the editors have grouped the pieces under general themes, beginning with the resistance to and rebellion against colonial rule and the restrictions it imposed on personal and collective freedom. Historical sequence is followed in chapters dealing with the political and economic protest of the nationalist parties and the rebellions that accompanied the period of decolonization and the eviction of European-dominated regimes. Finally the theme of protest is carried through into the postcolonial era when coups and local revolutions toppled African governments, often with greater violence than that required to overthrow the much more powerful colonial administrations.

Protest in Africa has taken many forms apart from organized political and military movements. It has come forth, as the editors point out, in religious movements, in trade unions,

and in literary output. It has been the product of traditional and of modernizing forces, of one man (or woman) whose charisma provided the focus for an uprising, and of spontaneous groups that rose rapidly in support of widely shared grievances. In the early years of colonial rule dissent centered around the fact of subjugation and the destruction of traditional society by the European conqueror. As Yves Person makes clear, however, in his illuminating chapter on Samori, the battle against the imperial armies was doomed to failure because of the technological superiority of the adversary. Partially as a means of mobilizing resentment and partially to offset the disintegration of indigenous culture, religious movements, centering particularly around Islam, became important vehicles for continuing the struggle against encroaching European influence. James Fernandez presents an excellent account of the role of a religious movement in his piece on protest movements in northern Gabon.

With the spread of education and the rise of a new generation of activist leaders who were fully capable of employing Western techniques of political organization, the nationalist parties became a major mode of protest in the postwar decades. Militant nationalism provided a new weapon for the eventual seizure of power. By means of the political party traditional loyalties could be combined with modern interest groups, such as the trade unions, to form an irresistible front demanding independence.

Unfortunately, the struggle for power, which left the inheritance of a dominant political party, left the way open for those groups that felt they had not achieved the full fruit of independence to embark on continued campaigns of protest. Crawford Young, in his admirable chapter on the Congo conflict, points up the place of social and economic dislocation as a factor in the rise of dissent throughout that country. Among the various discussions of military coups, James O'Connell's "The Fragility of Stability" presents a particularly incisive analysis of the fall of the federal government in Nigeria. He argues that dissension among the educated elite groups was an important factor eroding the power of the federal government.

An especially interesting facet of contemporary protest is dealt with by Ali Mazrui in his essay on "Socialism as a Mode of International

Protest." In it he seeks to illustrate that, in the case of Tanzania socialism, socialism was not only a means for creating a new domestic economic order but was also "an international protest against the constraints of economic relationships with others." The same point might, of course, be as forcefully made in the case of the Guinean regime of Sékou Touré.

Taken as a whole the volume is an excellent example of the service that can be rendered to students of Africa by bringing together scattered writings on a particular topic. The editors have selected well, although, as they themselves admit, it is not possible to put together examples of every type of African protest. The only unhappy note seems to come at the very end in Professor Mazrui's postlude, which is a brief attempt to lay the groundwork for a theory of protest. The impression is left that some bow toward theory was considered as imperative, but in this case, where such a wide canvas has to be painted, the attempt seems fuzzy and unconvincing.

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KARIN HAUSEN. *Deutsche Kolonialherrschaft in Afrika: Wirtschaftsinteressen und Kolonialverwaltung in Kamerun vor 1914*. (Beiträge zur Kolonial- und Überseegegeschichte, Number 6.) [Zürich:] Atlantis. 1970. Pp. 340. 42 fr. S.

RAINER TETZLAFF. *Koloniale Entwicklung und Ausbeutung: Wirtschafts- und Sozialgeschichte Deutsch-Ostafrikas 1885-1914*. (Schriften zur Wirtschafts- und Sozialgeschichte, Number 17.) Berlin: Duncker & Humblot. 1970. Pp. 309. DM 58. 60.

DETLEF BALD. *Deutsch-Ostafrika 1900-1914: Eine Studie über Verwaltung, Interessengruppen und wirtschaftliche Erschliessung*. (Ifö-Institut für Wirtschaftsforschung München, Afrika-Studienstelle. Afrika-Studien, Number 54.) Munich: Weltforum Verlag. 1970. Pp. 238. DM 44.

The near-simultaneous appearance of three works by young West German historians, who, working independently of each other, nevertheless address themselves to identical basic questions about the nature and structure of German colonial rule prior to 1914, signals a welcome renaissance of historical interest in Africa in the Federal Republic.

Karin Hausen's study of the dynamics of the

German system of rule in the Cameroons, exhaustively researched and documented on the basis of all major relevant sources available in both Germanies, offers important new facts and interpretations updating Harry R. Rudin's pioneer study *The Germans in the Cameroons* (1938) and questions some of the ideological simplicities of the East German school of colonial historians. The heaviness of her traditional German academic style and the rather pretentiously "scientific" manner in which her thesis is stated and restated throughout the book do not detract from the importance of her insights: In the Cameroons, as in other African colonies at a comparable state of development, there existed a basic conflict between the short-term goals of European economic interests—the maximization of profits by the untrammelled primitive exploitation of human and natural resources—and the long-term objectives of the administration, which sought the rational development of the colony for the benefit of the national economy of the mother country. While originally almost wholly subordinated to the interests of a handful of private firms, the Cameroons administration, according to Hausen, became a more autonomous factor during the last ten years of German sovereignty as its financial and personnel basis improved and acute conflict arose between commercial and plantation interests in which the government played the role of arbitrator. Nevertheless, the basic and explicit primacy of economic interests, a characteristic of German colonization, was not successfully challenged before 1914, and the indigenous population remained wholly subordinate to those interests.

Rainer Tetzlaff's able economic and social history of German East Africa stresses the same theme in its title, *Colonial Development and Exploitation*, and throughout the work. Tetzlaff concludes that despite the efforts of an exceptionally able and strong governor (A. von Rechenberg, 1906–12) to establish the primacy of long-term development goals and to maintain the position of the African producer vis-à-vis the planters, these goals were not achieved by 1914. He details the formidable settler opposition to all government measures that were seen to threaten their narrowly defined interests and investigates the rationalization of these interests in a brutal ideology of European su-

premacy and the organization and activities of the settler pressure groups that opposed Rechenberg's policies in the colony and in Berlin.

The study by Detlef Bald of German East African administration, interest groups, and economic development covers much the same ground. Despite its use of East African archival resources, the work suffers in comparison with Tetzlaff's through poor organization and lack of focus. Bald's verdict on the total effect of German colonial policies on the economic development of Tanganyika, and especially on the Rechenberg regime, is clearly more positive, as he is more inclined to accept the inevitability, and indeed the desirability, of the process of social and economic modernization.

These monographs illustrate effectively one of the central ironies of turn-of-the-century colonialism: European states and their colonial civil servants, professing capitalist-individualist ideology and objectives, consciously strove to remake the societies and economies of Africa by revolutionary methods of social engineering. While they did so entirely in the supposed interest of the mother countries' economies they unleashed dynamics in Africa's previously conservative societies that necessarily undermined the bases of European rule.

WOLFE W. SCHMOKEL

University of Vermont

URS BITTERLI. *Die Entdeckung des schwarzen Afrikaners: Versuch einer Geistesgeschichte der europäisch-afrikanischen Beziehungen an der Guineaküste im 17. und 18. Jahrhundert.* (Beiträge zur Kolonial- und Überseegeschichte, Nummer 5.) [Zurich:] Atlantis. 1970. Pp. 247. 29 fr. S.

Philip D. Curtin has already provided an analysis of the popular understanding of Africa in early nineteenth-century Britain in *The Image of Africa* (1964). Examining European attitudes toward black Africa prior to 1800, Urs Bitterli demonstrates that an "image" existed in the seventeenth and eighteenth centuries as well. In this study of the encounter between the white and black man, the Swiss historian utilizes the published descriptions of travelers, merchants, explorers, and scholars, and he focuses on a narrowly defined area of West Africa, the "upper" Guinea coast from the Senegal River to Sierra Leone.

The travel accounts that awakened Euro-

pean interest in Africa were generally inaccurate or distorted. Written from a purely Eurocentric standpoint, such literature portrayed Africans as barbarians or curiosities, and the evaluations made of indigenous cultures were negative in character. With the development of the ideal type of the "noble savage" in the eighteenth century, the European conception of the African became appreciably more favorable. This provided the leaders of the newly emerging abolitionist movement with an opening to extol the virtues of the African against the slave traders, but as J. Robert Constantine has shown in an article overlooked by Bitterli ("The Ignoble Savage, an Eighteenth Century Stereotype," *Phylon*, 27 [1966]: 171-79) the pro-slavery forces countered with an "ignoble savage" stereotype that deviated little from the seventeenth-century view of the black. The extensive discussion of the Negro's racial characteristics and his "lower position in the creation" (an alleged state of moral and intellectual inferiority to the white) had the positive result, at least in scholarly circles, that the black came to be regarded as a human being instead of a half-animal curiosity piece. Abolitionist propaganda, however, contributed far more significantly to a transformation in the popular image, and by the early 1800s Europeans had even become interested in bringing Christianity and "civilization" to the African.

This intellectual history of European-African relations affords many interesting insights but provides little that is particularly novel. Rightly Bitterli acknowledges his debt to Curtin, who already has covered much of this ground, although not as thoroughly. His familiarity with the contemporary travel literature is impressive, but a profusion of long, undigested quotations unnecessarily lengthens the study. Also, it would have been better if he had expanded the treatment to include the entire Guinea coast.

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LEWIS H. GANN. *Central Africa: The Former British States*. (The Modern Nations in Historical Perspective.) Englewood Cliffs, N. J.: Prentice-Hall. 1971. Pp. xii, 180. Cloth \$5.95, paper \$2.45.

Mr. Gann has that uncommon ability to

make one listen to him, even when the world view he presents is unpopular. On each occasion that he has considered central African history he has raised eyebrows, and he does so again in this brief book. He mostly writes about Europeans in that part of the world; Africans appear in his pages only to create situations in which white men are actors. (In his bibliography he makes the time-saving and astounding statement that the "anthropological literature concerning Central Africa is so extensive that we cannot refer to it here.")

Nevertheless, what he wants to write about he does extremely well and with enormous scholarship. The erudite analogy, the occasional production of a surprising fact, his obviously sure grasp of the material side of life there, all combine to make his writing attractive. There continue to be minor infelicities of style and syntax that betray his Germanic origin and that seem to have escaped his editor's notice. (The dust jacket evidently was prepared by someone unacquainted with the text, for there no longer is a Nyasaland, and the United Nations agreed in 1965 that Rhodesia is not an "autonomous nation.")

Two other minor points: Mr. Gann minimizes the degree of African urbanization in these three lands, and he believes that President Banda was a leading figure in the 1945 Pan-African Congress. There is no evidence for the latter and plenty against the former assertion.

Also, I think Mr. Gann is unfair when he says that colonels earn only six times as much as white privates in the Rhodesian army, but their counterparts in the Malawi Regiment earn twenty-three times as much as black privates. Surely, he should have indicated that when he wrote all the colonels in both countries were white, while only in Malawi were all the privates black. Black Rhodesian privates surely were the ones to compare with. I think Mr. Gann would agree that "equal pay for equal work" is not a Rhodesian strong point.

Nevertheless, having said all this, I recommend the book for introductory courses in central African history. In less than two hundred pages it moves adeptly from cave men to Mr. Ian Smith. Gann presumes a great deal in some places and is embarrassingly explicit in others, but he conveys an atmosphere. Everyone in

the field has been stimulated by his work at one time or another, and it is useful that a revisionist of the revisionists should restate his case at this time.

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STANLEY SHALOFF. *Reform in Leopold's Congo*. Richmond, Va.: John Knox Press. 1970. Pp. 195. \$5.95.

Although Professor Shaloff has not fulfilled the promise of his title by treating fully either the Congo reform movement in Britain and the United States or the impact of this effort in Africa, he has written a competent history of one of the groups that participated in the struggle on both fronts, the American Presbyterian Congo Mission (APCM). His account of conditions in the Congo is limited to the Kasai where the mission was located, and he emphasizes, though he is careful not to exaggerate, the contribution of the APCM to the reform agitation. The study is based on the records of the mission society and of the British reform groups, as well as on U.S. State Department and British Foreign Office documents.

Faced with the cruelties of Leopold II's rubber system, the American Presbyterians spoke out publicly at a time when their Catholic counterparts defended the regime and the British Protestant missionaries remained silent. This testimony was used by humanitarians in Britain in the campaign to force diplomatic action against the Congo Free State, an effort spearheaded in the United States by the board of the APCM. While the state retaliated by refusing additional mission sites, the author maintains that the APCM's defense of native rights contributed to the success of its evangelism by helping to win the Africans' confidence.

The study contains material of interest apart from the story of the reform effort. While the mission was financed by the racially segregated Southern Presbyterians, its personnel was integrated. The author demonstrates that this was less an indication of the softening of racial attitudes than of the persistence of pre-Civil War colonization schemes. An account of the progress of the missionary effort is provided as well as an analysis of the reasons for its varying success with diverse tribes and social groups. The

work is thus a useful supplement to Ruth Slade's *English-Speaking Missions in the Congo Independent State* (1959).

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Catholic University of America

ANTHONY NUTTING. *Scramble for Africa: The Great Trek to the Boer War*. New York: E. P. Dutton and Company. 1971. Pp. 454. \$10.00.

The book's title is misleading since it promises a study of interrelations between events in Southern Africa and the "scramble" elsewhere. Mr. Nutting deals only slightly with the continent as a whole even though he attempts to make such connections in a short introductory segment and with brief references throughout the work. A much narrower focus was necessary in order for the book to accomplish its stated purpose. What the author has produced is a generalized journalistic account dealing primarily with Boer-British tensions in the last quarter of the nineteenth century. The occupation of the lands north of the Limpopo constitutes a strong subordinate theme, and Mr. Nutting seems most comfortable when writing of Cecil Rhodes and his associates. Among the most interesting sections is that devoted to the Jameson Raid and its aftermath.

South African history lends itself to romantic treatment, and this combined with Mr. Nutting's writing facility masks very serious flaws in the book. Lacking a sharp thematic focus, the author tends to allow his account to wander. If the reader is interested in Portuguese activity in central Africa, he is unnecessarily moved backward and forward in time and shifted from the Zambesi to the Congo and finally to Nyasaland. Other annoying habits of the author are his use of questionable oversimplifications. Many key generalizations are stated without benefit of supporting data. Two examples indicate the effect of this upon the book's veracity. Was it always the avowed British policy in South Africa to "expand British dominion and to swallow up all who stood in the way"? Further, if one believes the statement that "Rhodes not being in a hurry was a contradiction in terms," then the Jameson Raid is easily understood, but much of Rhodes's policy before 1895 becomes inexplicable. Other generalizations, such as those concerning the

Da Cruz *prazo*, suggest that the author was unfamiliar with the latest monographs on certain subjects.

The extensive bibliography indicates that the author worked with numerous private and public collections. It is, therefore, very difficult to understand why he chose not to use footnotes. By his decision he deprived other scholars from sharing fully in his research and rejected a means that could have been used to defend his presently unsupported generalizations.

In summation, this is a book for the uncritical general public. It is too elementary and flawed to be of great use to persons with any considerable knowledge of Africa.

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ASIA AND THE EAST

ALEXANDER BARTON WOODSIDE. *Vietnam and the Chinese Model: A Comparative Study of Nguyễn and Ch'ing Civil Government in the First Half of the Nineteenth Century*. (Harvard East Asian Series 52.) Cambridge, Mass.: Harvard University Press. 1971. Pp. 358. \$9.95.

Foreign policy and military crises have a way of exposing gaps in our scholarly and intellectual armor. The Vietnam War has been no exception, and while there is hardly a shortage of prescriptive articles and pamphlets and while background studies have been slowly forthcoming, very little attention has been given to the systematic study of historical and contemporary Vietnamese institutions. While the relationship between China and Vietnam is correctly understood to be a crucial factor for reckoning with both societies and for comprehending in particular the latter, the bibliography for the field has been scant. In the wake of a war with Japan that posed a massive threat to the West and of an occupation and then a partnership that offered new challenges, American scholarship devoted itself to Japan; and a sophisticated generation of Japanologists emerged to take their places beside and eventually succeed an earlier generation of missionary and diplomatic corps writers. The inundation of China by missionaries, business interests, adventurers, and the like in the late nineteenth and

early twentieth centuries and the perceived threats in the Nationalist revolution and the Communist revolution of the past five decades has led to an evolution from romantic writings on China to a massive corpus of descriptive, scientific, and theoretical literature. Perhaps the same may now be expected for Vietnam. But, aside from the reluctance of young Asianists to enter fields of research that may unwittingly assist the war effort, the formidable inventory of research tools required for in-depth research on Vietnam makes qualified scholars scarce. Alexander Barton Woodside, however, has the qualifications. In this work he utilizes his knowledge of Chinese, Vietnamese, and Japanese to present a penetrating look at the institutions, traditions, and attitudes that governed nineteenth-century Vietnam and that determine much of what is happening today.

In comparing the civil structures of China and Vietnam, on the basis of Chinese and Vietnamese historical records, government statutes, popular and classical literature, he brings into focus the similarities and differences of the two societies as well as the influence of the former upon the latter. The comparison becomes a classical study in the modification of the Chinese cultural pattern in a Southeast Asian environment. Experience with comparable studies of Korea and Japan leads to the expected conclusion that Vietnam never succeeded in completely conquering the difficulties of domesticating the Chinese institutional model and furthermore that this model never completely stifled Vietnamese ingenuity. Woodside develops these ideas in a broad and thoroughgoing manner. Among the features examined are the tensions between the Vietnamese environment and the imported Chinese institutions, the dualism of the Vietnamese monarchy, certain recurring divergences in social structure and social ideology, the impact of narrowly channeled Confucian acculturation upon the political options of disenchanted intellectuals, and a wide range of general and specific institutional comparisons.

Beyond being a work absolutely indispensable for insight into the evolution of Vietnam's political society and style, the questions raised and research method may well be applied to other societies contiguous to China and even to

special areas that existed within the borders of China itself.

STANLEY SPECTOR
Washington University

HERRLEE G. CREEL. *The Origins of Statecraft in China*. Volume 1, *The Western Chou Empire*. Chicago: University of Chicago Press. 1970. Pp. xiv, 559. \$17.50.

This is the first volume of an ambitious project to study the origin and evolution of traditional China's political institutions, which, as Professor Creel points out, constituted one of the most successful and long-lasting forms of government devised by man (until its eclipse in the present century) and which have contributed in ways that are seldom realized to the organizational principles of the modern bureaucratic states in which we now live. The subject of the present book is the government of the Western Chou (traditionally dated from 1122 to 771 B.C.), the formative period out of which the classical civilization of China emerged in the latter part of the first millennium B.C. Confucius looked back to the founders of the Chou dynasty as his chief source of inspiration, and the idealized picture of their deeds and of the institutions they founded has been a source of ethical and political norms that have had incalculable influence on Chinese society for many centuries. Though modern scholarship rejects the Confucian idealization, the reality that lay behind it remains a most important object of inquiry if we are to understand what Confucianism was all about and, more generally, the origins of Chinese social and political forms.

Unfortunately the sources for getting at this reality directly are extremely limited, as Professor Creel shows in the appendix devoted to this question. Apart from later works referring to Western Chou, which must always be more or less suspected of anachronisms, we have only the text proper (as opposed to the commentaries) of the Book of Changes, a few portions of the Book of Documents, and the Book of Poetry that can with reasonable confidence be accepted as literary works from before 600 B.C. Besides this there are inscriptions on bronze vessels. Since many of these were cast to commemorate particular events, such as bestowals by the sovereign on his vassals, they are of the

highest relevance as historical documents. Unfortunately, apart from questions of authenticity, they are often only partly decipherable and even when we can feel fairly sure that we can read the words, their significance may be very obscure and open to varied interpretations, because we know so little of the background.

Professor Creel shows an admirable determination to rid himself of preconceptions based on the traditional views of the Confucian school and modern scholarly dogmas and to concentrate directly on what Chou texts themselves tell us. Unfortunately to make a picture at all out of such meager and fragmentary material requires a great deal of imaginative extrapolation, and I must confess that I find many of Professor Creel's extrapolations, based on what he thinks must have been the case from his views of human nature and Chinese nature in particular or from comparisons with early European feudalism, much less convincing than more conventional extrapolations based on what we know of Chinese society in the immediately following spring and autumn and Warring States periods. *Argumentum ex silentio* gets worked very hard by Professor Creel, and far-reaching conclusions are sometimes drawn from an interpretation, which seems by no means impregnable, of a single text.

It is surprising that more attention has not been paid to what archeology, apart from newly discovered written texts, has to tell us. For example, the very structure of a Shang dynasty city, from which later cities of Chou times evolved in an uninterrupted way, has been held to point to a sharply divided society with a small, bronze-using, literate, ruling aristocracy living and pursuing its administrative and ceremonial roles in walled citadels, physically and spiritually separated from the mass of the peasantry still living at a neolithic level in terms of material culture. (See Kwang-chih Chang, *The Archaeology of Ancient China* [rev. ed.; 1968], 307 ff.) Professor Creel has surprisingly little to say about social structure and practically nothing about the economic base of society, but he does attempt at various points to undermine the traditional view that a hereditary aristocracy played an exclusive or

predominant role or even that it really existed at all in early Chou times. Space will not permit a detailed discussion of his arguments, but I feel that they fall far short of making even remotely plausible such a radical revision of the orthodox opinion. Professor Creel's small attention to the religious and ritual side of Chou political institutions also seems strange.

In short I feel that Professor Creel's approach is too personal to provide an authoritative synthesis of the present state of knowledge on this subject. Nevertheless his book offers many challenges to received opinion and will, I am sure, provide much in the way of stimulus to further research.

E. G. PULLEYBLANK

University of British Columbia

DAVID TOD ROY. *Kuo Mo-jo: The Early Years*. (Harvard East Asian Series 55.) Cambridge, Mass.: Harvard University Press. 1971. Pp. 244. \$7.50.

With this little volume David Roy has begun what promises to be a superb biography of one of the most influential Chinese intellectuals of the twentieth century. Kuo was born in 1892 and received his early education in Szechuan during a period when the curriculum was changing to respond to Western influences. In 1913 he went to Japan, where he enrolled in medical school, but focused his energies primarily on studying Chinese and Western philosophy, translating Western literature, and writing poetry and other creative works. From 1921 to 1924 Kuo helped organize and lead the Creation Society, one of the chief agencies of the literary revolution. Roy lucidly describes all these phases of Kuo's life and intends to continue the story after 1924 in a later volume.

The question underlying the entire book is how Kuo came to accept Marxism. Roy argues that he held certain ideas derived from Chinese tradition, particularly from Chuang Tzu and Wang Yang-ming. These concepts, especially pantheism, were strengthened and extended by ideas Kuo found in the works of Tagore and of Goethe, Whitman, and other Western writers, which pushed him into the mainstream of Western Romanticism. Marxism seemed to offer Kuo the possibility of a social organization in which his essentially romantic values and goals might be realized.

The important point is that the acceptance of Marxism neither marked a sharp break in Kuo's development nor necessitated a complete rejection of the Chinese past or of his own earlier convictions. Roy is properly wary about generalizing on the basis of Kuo's individual experience, but many readers will find in this excellent book a confirmation of the notion that the strength of Chinese Marxism lay in the many strands of continuity that linked it with elements of the Chinese past.

JAMES E. SHERIDAN

Northwestern University

S. A. M. ADSHEAD. *The Modernization of the Chinese Salt Administration, 1900-1920*. (Harvard East Asian Series 53.) Cambridge, Mass.: Harvard University Press. 1970. Pp. xi, 280. \$8.75.

This is an important and lucid study of modernization and imperialism in twentieth-century China. It takes as its subject the salt administration, which was a vital part of the traditional Chinese fiscal system, since after land salt was the most consistently taxed commodity. As Adshead so ably demonstrates, between 1900 and 1920, but particularly after 1913 when as security for the Reorganization Loan it came under the control of a staff of foreign inspectors headed by Sir Richard Dane, the salt administration was almost thoroughly modernized in all aspects except the technological. Specifically, the marketing of salt was speeded up and expanded as a by-product of railway construction. The tax collection was simplified and at the same time made more comprehensive by taxing at the source, the salines, rather than, as formerly, along the transportation routes. Moreover, the tax receipts, much of which previously had been siphoned off by provincial authorities before they reached the capital, were greatly centralized. Finally, the salt administration itself was transformed from a corrupt, generalist service deeply involved in politics into a noncorrupt, nonpolitical, specialized service.

Adshead's study deserves the attention of comparative historians as well as historians of China. For the latter, in addition to what it says about the salt administration, it contributes to a better understanding of the Yuan Shih-k'ai and subsequent warlord regimes in

Peking, for whom salt had become the primary source of revenue. For the former, it is important as a case study of institutional modernization, for which Adshead offers a narrow and precise definition that others may find useful, and of imperialism, and of the two together: modernization under the aegis of imperialism. He sees the reorganized salt administration as an example of synarchy, which he characterizes as "the predominant instrument of European imperialism in Asia," and explicitly compares China with Persia and the Ottoman Empire. However, the discussion at the end about the overall benefits of synarchy is too brief to be convincing.

In Adshead's account, Dane's accomplishments—reminiscent of Sir Robert Hart's in the Maritime Customs Service—stand out clearly. The ways he went about them, however, are sometimes obscure. In particular, his success at rationalizing the salt administration needs explanation. For example, how did he eliminate corruption? How did he depoliticize the staff? How did the Chinese and the foreigners on his staff get along? Since Dane could have achieved little on his own, much more might have been said about his staff. A map showing the salt producing areas and the distribution divisions would have been welcomed too.

EDWARD RHOADS
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YOJI AKASHI. *The Nanyang Chinese National Salvation Movement, 1937-1941*. (International Studies, East Asian Series. Research Publication, Number 5.) [Lawrence:] Center for East Asian Studies, University of Kansas; distrib. by Paragon Book Gallery, New York. 1970. Pp. 211. \$6.50.

During this period of new initiatives in Sino-American relations, the political significance of Southeast Asia's twelve million Chinese remains obscure. Despite changes over the past thirty years, Yoji Akashi's study is highly pertinent.

The reader should not be misled by the title. Though "Nanyang" is translated throughout in its most literal sense of "South Seas," the author uses the term interchangeably with "Southeast Asia." Furthermore, the emotive sense of "*chiu-kuo*" would be more accurately

conveyed by the imperative "Save the nation!" than by "national salvation"—though the reviewer has also resorted to this bland term for syntactical convenience. Assassinations and other means of intimidation used to enforce anti-Japanese boycotts and pro-Chinese fundraising drives reflected the sense of total commitment that motivated the movement's organizers. But coercion was generally unnecessary. Without the voluntary financial contributions of the much-maligned "Jews of Southeast Asia," the author suggests, Chiang Kai-shek may have been unable to sustain his war effort prior to Pearl Harbor.

Though Akashi traces the patriotic movement among Southeast Asian Chinese from the anti-Manchu tide of the early twentieth century, he seems unfamiliar with the dramatic upsurge in the national salvation movement on the Chinese mainland that immediately preceded his saga. The Southeast Asian scene, on the other hand, is treated in all its richness and diversity. The scattered, leader-poor Chinese of Indonesia were less responsive than the relatively cohesive Malayan community. Repression by pro-Japanese Thai authorities contrasted with the permissiveness of French officials in Indochina. Militancy of first-generation, China-educated emigrées contrasted with the caution of long-established settlers. Diplomats from China feuded with local leaders. Regional, clan, and party loyalties vitiated the movement's strength.

In spite of all this, Chinese nationalism transcended particularistic loyalties. This fundamental truth is a starting point for understanding Southeast Asia's Chinese today.

JOHN ISRAEL
University of Virginia

ALBERT M. CRAIG and DONALD H. SHIVELY, introduced and edited by. *Personality in Japanese History*. (Published under the auspices of the Center for Japanese and Korean Studies, University of California, Berkeley.) Berkeley and Los Angeles: University of California Press. 1970. Pp. x, 481. \$8.75.

On the occasion of his sixtieth birthday Professor Edwin O. Reischauer, former ambassador to Japan, has been presented a volume of essays by his former students. The general theme of the volume is biography, and the several

studies offer the reader a variety of examples from Japan's past. The specific time span covers the years from the early Tokugawa period to the twentieth century. In addition to an introduction, written by Professor Craig, there are the following essays: John C. Pelzel, "Human Nature in the Japanese Myths"; John W. Hall, "Ikeda Mitsumasa and the Bizen Flood of 1654"; Donald H. Shively, "Tokugawa Tsunayoshi, the Genroku Shogun"; Thomas C. Smith, "Ōkura Nagatsune and the Technologists"; Tetsuo Najita, "Ōshio Heihachirō"; Conrad Totman, "Political Reconciliation in the Tokugawa Bakufu: Abe Masahiro and Tokugawa Nariaki, 1844-1852"; Robert K. Sakai, "Shimazu Nariakira and the Emergence of National Leadership in Satsuma"; George M. Wilson, "The Bakumatsu Intellectual in Action: Hashimoto Sanai in the Political Crisis of 1858"; Albert M. Craig, "Kido Kōin and Ōkubo Toshimichi: A Psychohistorical Analysis"; Marius B. Jansen, "Mutsu Munemitsu"; George Akita, "The Other Itō: A Political Failure"; Akira Iriye, "Kayahara Kazan and Japanese Cosmopolitanism"; Peter Duus, "Nagai Ryūtarō: The Tactical Dilemmas of Reform"; and Howard S. Hibbett, "Akutagawa Ryūnosuke and the Negative Ideal."

The bulk of the essays are concerned with political actors, with the exception of Pelzel who tries his hand at an archetypal analysis based on the Japanese myths and Hibbett who offers a sensitive portrayal of a major literary figure. Unfortunately, the title of the book promises far more than the text delivers, but the several contributors are, perhaps, not to be blamed for the expectations of the editors. Despite Craig's rather derivative introduction, in which the reader is offered a short course in personality theory, the majority of the essays disclose only an enduring—indeed an abiding—belief in the claims of political narrative as a heuristic device. Yet it is, I think, the intention of such an elaborate introduction either to equip the volume with a unified theoretical purpose or, at the very least, to hint at the possibility that such a purpose is silently shared by the several authors. But if it is not either, then Craig has written an introduction for another book. What we have here is not an examination of Japanese personality, but a collection of biographical essays on Japanese

personalities. Few of the essays, in fact, are directly concerned with the question of personality and the numerous problems raised by the attempt to rescue a life from history.

Where such attempts are made the results are discouragingly disappointing. Craig, for example, sandwiches fairly conventional and very accessible anecdotes about Kido and Ōkubo (two major figures) between heavy psychological assumptions ranging from mother fixation to experiences in peer groups. But the anecdotes already tell us what the apparatus is made to yield. Yet if Craig's study reveals the pitfalls of seeing anecdote as psychohistory, Akita demonstrates, with almost uncommon skill, the utter failure of such an approach. Here is psychohistory at its most vulgar level. In what can only be described as a tour de force in drugstore psychiatry, he offers a second-rate view of what he considers a second-rate politician. Even a "political failure" deserves something more from his biographer.

But if Craig and Akita show us what we must avoid if we are to write psychohistory, Smith, writing on a technologist, discloses new possibilities for archetypal analysis; Najita, on a genuine revolutionary, demonstrates forcefully that some personalities are politically more significant (if not interesting) than others; Jansen on Mutsu shows, skillfully I think, what kind of "straight" political biography might still be written on a political figure who failed to satisfy his ambitions, without subjecting him to the indignities of freshman psychology; and Hibbett, in his subtle handling of a self-conscious writer, delivers the real promise of psychohistory. In these studies the mode of analysis is appropriate to the specific concentration in personality that the authors have chosen to dramatize the historical meaning of a life. Yet in deciding on a project they have found the moment and the experience that in fact confers meaning on the life of a personality in history.

Naturally each reader will find his favorites in this volume. But he will also find that the range and variety, despite the number of contributors, is surprisingly limited. Those who still believe in good old political biography, the grand sweep of anecdote, and the security of chronological design will, nevertheless, find in this volume a familiar friend and a reaffir-

mation of some of their most cherished historical pieties.

H. D. HAROOTUNIAN
University of Rochester

DAVID C. COLE and PRINCETON N. LYMAN. *Korean Development: The Interplay of Politics and Economics*. (Written under the auspices of the Center for International Affairs, Harvard University.) Cambridge, Mass.: Harvard University Press, 1971. Pp. xiii, 320. \$12.00.

Now it is almost inevitable that studies like this one undertaken by Cole and Lyman will be viewed by some scholars in a highly critical fashion. The first draft of *Korean Development* was prepared in 1967-68, when the authors were at the Center for International Affairs at Harvard. Not only has the Center been attacked as a sort of cultural outrigger on the American imperialist canoe, but also the authors fit perfectly the prototype dubbed "scholar-politicians." In this dissenting view, there is "an ideology of modernization" designed to play down "American aggression and exploitation" and to play up "American benevolence in assisting Asians. . ." (see, for example, Edward Friedman and Mark Selden, *America's Asia: Dissenting Essays on Asian-American Relations* [1971]).

A more orthodox description of the authors is that they are skilled development specialists looking at Korea, rather than Korean specialists looking at development. Cole, an economist, is presently with Harvard's Development Advisory Service in Indonesia; Lyman, a political scientist, has returned to AID in Washington. In their work they relied heavily on Korean informants, particularly those affiliated with the Politics and Society Project in Korea; they consulted the best-known American experts on Korea; their work was first aired in seminars at MIT and Harvard. The finished product admirably supplements work done by "area" specialists such as Gregory Henderson, Hahn-been Lee, and John Kie-chiang Oh.

Cole and Lyman are concerned primarily with the period most marked by political and economic progress, 1963 to 1967. As a *Taehan Ilbo* editorial put it, 1966 was a "watershed of Korean history," in which "agony evolved into the amplitude of modernization." The study traces the interrelationship among three fac-

tors: political dynamics, national outlook, and economic structure. The authors quite explicitly focus on "development" (only one type of "modernization"), which provides economic growth by consensus rather than by coercion.

After a brief survey of antecedents (part 1), there is therefore not only emphasis on economic modernization (part 2) but also treatment of the struggle between totalitarian and democratizing influences (part 3). For the "area" expert and the historian, one of the most interesting chapters is on the climax of dissent, the Japanese ("normalization") settlement of 1965.

Although the critical period (1963-67) represented but a first step, it did, according to the authors, mark an era in which "the government had found the means for common interest and interaction between itself and the majority of the population. . ." (p. 239). This is not to ignore "Lingering Restraints" (chapter 11). Economic growth factors in South Korea have remained favorable. The remaining fundamental problem is political: the republic has yet to devise means for an orderly change of leadership.

The study provides ample documentation in notes (pp. 237-306), in a selected bibliography (pp. 307-12), and in an appendix of twelve tables (pp. 237-66).

ARDATH W. BURKS
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G. BHAGAT. *Americans in India, 1784-1860*. New York: New York University Press, 1970. Pp. xxvii, 195. \$10.00.

Professor G. Bhagat has exhaustively probed a wide variety of archival sources to tell the heretofore untold story of American trade with India in the period 1784-1860. The heart of the book actually concerns the period 1783-1812, during which time European preoccupation with the Napoleonic wars left the India carrying trade open to the enterprising Americans. It is the China trade that has always received the attention of historians, and Professor Bhagat is at pains to point out that this is a case of misplaced emphasis. He asserts that for much of the nineteenth century, the trade with India far surpassed that with China. In 1806-07, for instance, the United States trade with India

amounted to \$4,168,276 and that with China to only \$2,018,347. But, of course, the great days of the clipper ships were yet to come. A fundamental question, which has to be asked, is whether either trade in the early part of the century amounted to much. Total United States trade in 1806 amounted to \$102 million and that in 1807 to \$108 million. Thus the India trade amounted to somewhere around four per cent of the total and that with China to about two per cent.

Professor Bhagat traces the American trade with India from its rise and relative success in the immediate post-Revolutionary War era, through its decline in the post-1812 period, to its virtual extinction on the eve of the Civil War. He presents useful discussions of the American consular relations with India and of the techniques and inventory of trade. He has managed to identify most of the entrepreneurs and the vessels engaged in the India trade. The tale has a certain romantic appeal, and it is a shame that Professor Bhagat did not feel free to write with more verve and with a freer hand. Traders like Jacob Crowninshield, who purchased an elephant in Calcutta for shipment home, and Frederick Tudor, who made his fortune shipping ice to Calcutta, appear bloodless; and one can't help feeling that they really were not.

There is no doubt that Professor Bhagat has filled a lacuna in our knowledge of early American contacts with Asia. If his treatment lacks something in analysis, it at least supplies most of the factual data.

ROBERT A. HUTTENBACK
California Institute of Technology

NARENDRA KRISHNA SINHA. *The Economic History of Bengal, 1793-1848*. Volume 3. Calcutta: Firma K. L. Mukhopadhyay. 1970. Pp. iv, 172. Rs. 20.

This book is a sequel to the author's two-volume study *The Economic History of Bengal from Plassey to the Permanent Settlement* (1956 and 1962). Sinha's cautionary statements in his preface—that his work lacks a sophisticated quantitative approach or any theoretical basis—are well taken, for these are among the principal failings of the book. Despite its broad title, the scope is limited mainly to a brief treatment of trade and finance. Many facts are

strung together, but there is little analysis. In fact, this book is another reminder of the difficulties of writing the economic history of India, particularly regional economic history, in the absence of a substantial body of historical research into economic and social history. Apart from a few classic works, detailed work in Indian economic history, especially on a regional basis, has only recently begun; and until the output of monographs increases, it is difficult to produce adequate syntheses. Sinha's book does not help much: it is based on a limited range of sources, and for some reason the author does not seem to have made any use of the available secondary literature. For example, the discussion of the agency-house system does not take into account S. B. Singh's *European Agency Houses in Bengal, 1783-1833* (1966). Indeed, the list of sources used makes no reference to secondary works at all.

There is, however, one interesting feature of the book that should be mentioned. This is Sinha's use of the judicial records of the Calcutta Supreme Court to help explain such topics as indigenous Indian banking, capital accumulation, and business enterprise. Although such sources will not be easy to use, they will provide important information in filling in one of the widest gaps in the field of Indian economic history—business history.

PETER HARNETTY
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ARABINDA PODDAR. *Renaissance in Bengal: Quests and Confrontations, 1800-1860*. Simla: Indian Institute of Advanced Study. 1970. Pp. 254. \$7.00.

The writing of Indian history has suffered greatly from an obsession with finding analogies to Western historical experience. One of the most persistent—and most misleading—of these analogies is the concept of a "Bengal Renaissance." A measure of Arabinda Poddar's scholarship is that, despite his acceptance of a worn-out concept for organizing his material, he has managed to produce an interesting book. As his subtitle implies, his concern is to analyze the ways in which a number of Bengal intellectuals, whose careers made confrontation with the West inevitable, came to terms with the intrusive, and, at times, strident challenges posed by its political domination in the first

half of the nineteenth century. What the intellectuals had in common was that, unlike their counterparts in India and elsewhere in the twentieth century, they accepted, and even applauded, the political and intellectual domination of the Western world. And all of them, although by very diverse standards of judgment, sought a synthesis of what they considered to be the best and most valuable features of Indian and Western civilizations. That they were ultimately unsuccessful, or as Poddar puts it, "that the tasks of renaissance have largely remained unfulfilled," was probably inherent in the attempt. A genuine cultural synthesis has surely seldom occurred through self-conscious effort.

Poddar is only slightly hampered by the "renaissance" idea because he in fact uses the currently fashionable concept of cultural alienation. Early in the nineteenth century, he argues, the Bengal intellectuals who made creative contributions had become ambivalent toward their culture. Restless and uncertain in their own intellectual home, they looked to the West for security. This necessitated a form of self-assertion, of nonconformism, that, Poddar suggests, was European in origin and wholly alien to India. He inevitably credits Ram Mohan Roy, no doubt correctly, with making the first breakthrough from what he calls the "introvert-masochist" Hindu world to the "extrovert-hedonism" of the West. Radhakanta Dev, who strenuously opposed Roy's attempts to reform Hindu society, nevertheless, in Poddar's analysis, moved in the same direction through his very involvement in opposition activities as well as through his social contacts with the English, whose conquest of Bengal was the source of his family's wealth and eminence. The use of the English language was also a prime source of his alienation, forcing dependence upon the foreign rulers for both economic and intellectual sustenance.

Thus Poddar's conclusion is that the Bengal intellectuals failed to transform their society because of estrangement from their own world through their emotional ties with Western culture. The process worked both ways: the intellectuals moved away from their own cultural base, while the new world they inhabited was inaccessible to the Indian masses. This is not a very novel argument, but it is worth restating

as a corrective to nationalist versions of Indian history, which fail to note the ambiguities and contradictions in the Indian response to the West.

AINSLIE T. EMBREE
Duke University

KENNETH L. GILLION. *Ahmedabad: A Study in Indian Urban History*. Berkeley and Los Angeles: University of California Press. 1968. Pp. viii, 195. \$7.00.

Ahmedabad, India's sixth largest city, is not a typical Indian metropolis. It has been a trading city from time immemorial. Ruled mostly by a hereditary plutocracy, Ahmedabad has had a strong corporate tradition. It has hardly ever depended on the parasitic exploitation of its countryside. In the nineteenth century it survived the British industrial invasion that ravaged the traditional manufacturing towns of Dacca and Murshidabad; in fact, during this period it improved its economic position. The secret of this success, says Dr. Gillion, is due in part to the peculiar cultural characteristics of the Gujaratis, who are the least worldly of all Indian peoples. An indigenous financial and mercantile elite led Ahmedabad's industrial growth in the nineteenth century through a slow and selective adaptation of Western technology. Yet in Ahmedabad during much of the nineteenth century there was no cultural or intellectual renaissance; there was hardly any Western-educated elite, nor were there even any Scottish mercantile and financial houses. Its social and political institutions remained second-rate. The performance of its municipal government was so poor that in 1909 the government had to intervene to appoint a municipal committee and an executive officer to run the local government. Thus, until recent times, there was hardly any correlation between Ahmedabad's economic success and sociopolitical change.

Dr. Gillion has written a most readable and sweeping study. Based on published sources, it draws heavily from the Ahmedabad gazetteer and the studies (in Gujarati) of Maganlal Vakhatchand and Ratnamanirao Jhote. While the period 1817-1916 has been examined in some detail, the chapter on the Gandhi-Patel era in Ahmedabad is rather superficial. Dr. Gillion is at his best in summarizing and neatly organiz-

ing facts about Ahmedabad's urban history, but his analysis is rather weak. We are unable to understand why the nineteenth-century mercantile elite was unable to give Ahmedabad a sound local government. Why is it that the Ahmedabadis did not produce a bourgeoisie with a will for power over the Gujarati society? As a preliminary excursion in Indian urban history this volume is welcome.

BRIJEN K. GUPTA
University of Rochester

B. B. MISRA. *The Administrative History of India, 1834-1947: General Administration*. (Completed under the auspices of the Indian Institute of Public Administration, New Delhi.) New York: Oxford University Press. 1970. Pp. xv, 672. \$19.25.

Professor Misra has provided students of Indian history with a detailed, careful, and exhaustive study of the growth and proliferation of the administrative organization and system of the government of India from the beginning of the nineteenth century—when systematic government developed pursuant to the Cornwallian reforms—down to independence. In doing so, he has illuminated the ways in which the various branches of the executive government came into being and grew, often in response to an arcane inner logic. These developments are traced in minute detail in the second chapter in particular and in several other portions of the text as well. There is also a detailed, but not particularly critical, survey of the growth and transformation of the public services of India. That is, of course, ground that has by now been covered more than once.

Dr. Misra has also included a lengthy chapter on the development of the Indian councils after 1833. This is very well-worn ground and his account, not especially relevant to the title of the book, breaks no new ground even though he presents a thorough and balanced study.

One of the most useful sections of the book is the chapter on "The Administration of Indian Finance," in which Dr. Misra covers the ground fully and lucidly from 1834 to 1947. This complex subject has been slighted in most texts, and his account brings together in one place a wealth of valuable data.

The chapter on "Administration of Reve-

nue" is equally exhaustive, but covers material that has, by now, been treated at some length in several published studies. Perhaps no single volume on the topic can claim to be as detailed in its treatment as is Misra's, but taken together the extant accounts rob his version of freshness.

The final sections deal with the administration of justice, civil and criminal procedure, and the courts, as well as with local self-government. The latter, in Dr. Misra's hands, is an epitome of technical, administrative history detailing all of the rules and regulations, for example, under which a person could qualify for election as a commissioner under the Madras Act V of 1878 given in the text. But there is little evaluation of how such institutions actually worked.

The volume under review is a monument to careful and detailed scholarship of the older variety. It is in many ways a gold mine of reference material for those who want to know what were the acts, rules, and regulations upon which the government of India—or of the provinces—operated at any given point in time. Many questions about the ways in which those institutions worked in an Indian setting are not raised. Nonetheless, we must be grateful to Dr. Misra for his scholarship and his industry.

ROBERT I. CRANE
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I. J. CATANACH. *Rural Credit in Western India, 1875-1930: Rural Credit and the Co-operative Movement in the Bombay Presidency*. Berkeley and Los Angeles: University of California Press. 1970. Pp. xi, 269. \$8.75.

H. C. L. MERILLAT. *Land and the Constitution in India*. (Southern Asian Institute Series.) New York: Columbia University Press. 1970. Pp. xiv, 321. \$10.00.

Land policy is without doubt one of the most important problems of modern Indian studies. Therefore it is doubly pleasant to welcome these two volumes, both of which are concerned with rural development, although they are quite different.

Catanach's book is an elegant monograph, strictly defined, soberly balanced, and gracefully presented. He is concerned with the history of rural cooperatives—as distinct from agricultural banks—in Gujarat, western Maha-

rashtra, and the Bombay Karnatak. Much of the point of this study is an explanation of why the cooperative movement eventually failed. The utilization of honorary workers, the necessity of repaying cooperative loans at a specific date, anti-Brahman prejudice, and the fact that district banks invested in provincial banks instead of making loans to societies were all part of the picture. The district banks had strong connections with the money-lending classes who were the chief competitors of the cooperatives. The cooperatives were most needed by the poorer peasants, yet it was only the more well-to-do peasants who could furnish them with capital. In any case, the cooperatives were no substitute for adequate rainfall in the Deccan.

Both in its original research and its application of the results of pertinent investigations (by Eric Stokes, Bernard Cohn, Ravinder Kumar, Harold Mann, and Henry Orenstein, for example), this is an outstanding contribution. Catanach, a young New Zealand scholar, has not only measurably increased our knowledge of the workings of cooperative societies and conditions of rural indebtedness in what used to be called the Bombay Presidency, but he has made a permanent contribution to an increasingly sophisticated literature. Incidentally, one of the major values of this work is that it provides a basis for comparison with the Punjab cooperative experiments as recorded by Sir Malcolm Darling (*The Punjab Peasant in Prosperity and Debt* [1925]), and others.

Merillat's book has the advantage of factual accuracy, clear style, and an important subject. His conclusion that the agricultural sector in the modern Indian economy is not bearing a fair share of the tax burden for political reasons is probably correct. The book's announced purpose, however, of giving both lawyers and the general reader "some understanding of the social context in which Indian law relating to property rights in land has developed" is not fulfilled. Some may think the author would have done better to have written a treatise on contemporary Indian land law. In setting the broader problem, Merillat has not done justice to various social and economic elements, for example, the difficulties posed by the conflict of caste custom, British common law, Muslim law, and Hindu customary law. One

might add that it would be an unusual "general reader" who would cope with this discussion.

MARK NAIDIS

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SITA RAM SINGH. *Nationalism and Social Reform in India*. Foreword by BISHESHWAR PRASAD. Delhi: Ranjit Printers and Publishers. 1968. Pp. viii, 391. Rs. 30.

A number of useful general studies of the Indian social reform movement have appeared in recent years. All of them, from Charles Heim-sath and S. Natarajan onward, have emphasized the connection between social reform and resurgent nationalism and have attempted to encompass men as diverse as Rammohun Roy and Gandhi between the covers of a single volume. Sita Ram Singh follows in this tradition. Perhaps even more forcefully than his predecessors, he insists that "nationalism and social reform were interdependent," and that late nineteenth-century Hindu revivalism was neither "sectarian" nor politically "reactionary" (pp. 49, 128). No doubt some of the author's efforts to rehabilitate Tilak and the Theosophical Society can be attributed to his nationalist sympathies. But much of his energy is needlessly expended beating the dead horse of J. N. Farquhar. No one today seriously questions the reforming credentials of the Arya Samaj or Swami Vivekananda.

One might argue as well with Sita Ram's assertion that Hindu revivalism—"all-inclusive and all-absorbing"—was national in scope as contrasted with the "exclusive and separatist" (p. iii) character of contemporaneous Muslim thought (which he does not otherwise discuss). He likewise dismisses as simply orthodox the Sanatan Dharma associations. This is to overlook their novel organizational form and their contribution to the politicization of the Hindu religion. As Hindu is too readily equated to national, so too is orthodox too readily opposed to reformist. All these terms deserve more intensive and critical scrutiny than the author gives them.

Despite these shortcomings, however, this volume does contain much important new information. Sita Ram Singh fortunately does not confine himself to recapitulating the thought of the giants of Indian politics, but

surveys the activities of all the varied indigenous social reform agencies (missionary and government activities are excluded). By far the most valuable portions of the book are the topical chapters—drawn from the published but hitherto little-used proceedings of the National Social Conference and the social reform journals—which describe the progress of female education, marriage reform, and uplift of the depressed classes, together with such other diverse “causes” as temperance, the obscenity of the Holi celebration, and the suppression of temple prostitution. For those who want to know what was happening in the world of Indian social reform this book will prove a valuable introduction.

THOMAS R. METCALF
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JYOTIRINDRA DAS GUPTA. *Language Conflict and National Development: Group Politics and National Language Policy in India*. (Sponsored by the Center for South and Southeast Asia Studies, University of California, Berkeley.) Berkeley and Los Angeles: University of California Press. 1970. Pp. viii, 293. \$6.75.

Language has long occupied a crucial dimension in South Asian politics. Its political importance becomes especially salient when linked with either subnational or religious factors. Such was the situation in prepartition India as Urdu became aligned with the concept of an Islamic state. The creation of that movement, Pakistan, now finds itself struggling with Bengali nationalism.

Political fragmentation then is one possibility for a state with divergent and conflicting language groups. India would appear to be as likely a subject as Pakistan for linguistic irredentist movements. A continuing stream of scholarly and journalistic accounts predict just such a result. The classical case for the coming “Balkanization” of India remains Selig Harrison’s *India: The Most Dangerous Decades* (1960). Finally, after eleven years, a major rebuttal is available.

Professor Das Gupta goes much further than a mere rebuttal. In fairness to his framework, the rebuttal aspect is a minor concern. Employing a dynamic view of politics as an independ-

ent factor and using conflict theory, he makes a solid case for the contributions that language rivalry has made to national integration in India. Indeed, this almost completely reverses what the author labels the “Indian and the international prophets of despair” (p. 260).

I see two dimensions as central to the book. First, in a careful examination of language associations—primarily in northern India—his conclusions are similar to recent analyses of caste associations. Essentially, the organizational expression of what often is seen as a primordial attachment is comparable to an interest group. A pluralist society has equipped itself with demand structures, that is, representative associations, which have done a reasonably good job of coping with extremely difficult and complex problems. The theoretical underpinning of this view is the author’s argument, throughout the book, that social cleavages do not necessarily determine political cleavages. Thus, the interaction of competing interest groups in the language sector does not necessarily lead to fragmentation. To rephrase a previous point, politics can serve to unite as well as divide.

Second, a developmental perspective flows from this conception of politics as an independent variable operating in a context of conflict. Language interest groups have served an important function in mobilizing people, in serving as an important link with the centers of power, and in transforming the primordial qualities so as to result in an institutionalized part of the democratic fabric rather than in irredentist movements. In short, institutionalized conflict, in this case, appears not only to be congruent with national integration, but with the growth of democratic norms.

Despite the excellence of the documentation and the convincing theoretical arguments, this book really opens a subfield for further work rather than serving as a definitive statement. More research is necessary in northern India, let alone in the almost untapped Dravidian language areas. Moreover, there is a tone of optimism regarding language settlements thus far reached that may have to be tempered in line with future developments. Nonetheless, the book is essential for South Asian specialists of all disciplines, and is highly recommended for

any scholar concerned with historical or modern aspects of social and political development.

PAUL WALLACE
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Columbia

JOHN PATRICK HAITHCOX. *Communism and Nationalism in India: M. N. Roy and Comintern Policy, 1920-1939*. (Written under the auspices of the Research Institute on Communist Affairs, Columbia University.) Princeton: Princeton University Press, 1971. Pp. xiv, 389. \$12.50.

At the heart of this well-constructed book is the sad story of a revolutionary's long and fruitless quest for power. In his opening chapters, dealing with M. N. Roy's comet-like appearance in and departure from the Moscow-directed Communist International, Haithcox supplements but does not supplant Gene Overstreet and Marshall Windmiller's *Communism in India* (1959). Where Haithcox stresses Roy's theoretical contribution to the Comintern's 1920 debate on strategy in colonial areas, Overstreet and Windmiller suggest that his personal ambition to control the pipeline of authority and funds from Moscow to India via Berlin was the main reason for his vigorous involvement in this debate. Haithcox similarly "leans to one side" in reviewing Roy's bizarre 1927 mission to China to implement a Comintern "line" he had openly disputed with Stalin. Neither treatment considers the possibility that Stalin, by sending Roy on a hopeless mission, was using him primarily as a pawn in his chess game against his rivals in the Soviet Union—an explanation that seems to be confirmed by Haithcox's view that Stalin expelled Roy from the Comintern in 1929 because of his closeness to Bukharin.

In the second (less tightly organized) half of the book Haithcox breaks new ground, beginning with a detailed account of Roy's daring return to India and evasion of the police for seven months. He shows Roy's remarkable success in directing his followers from his jail cell from 1931 to 1936, and gives new evidence for the Royists' influence on the Congress Socialist party from its founding in 1934 until he ordered them to leave and attack it in 1937. Roy's futile attempt to recapture leadership of the communist movement, and his 1939 expulsion

from the Congress as a result of Gandhi's bloodless purge, are well handled.

Haithcox has marshaled an impressive amount of material, drawn from the Roy Archives in Dehra Dun, from a wide range of published English-language sources, and from interviews and correspondence with sixty-one of Roy's associates. Photographs of Roy in the United States, Mexico, Moscow, China, Switzerland, and India add to the book's considerable documentary value. He has not dealt as penetratingly as he might have with the larger conceptual and comparative problems implied by its main title: the complex and changing interactions among revolutionary communist, evolutionary socialist, and a variety of nationalist ideas and techniques being tried out in the U.S.S.R., China, and India during the interwar years. Let us hope he will give us these fruits of his deep knowledge of these interactions in a revised edition of this volume, correcting at the same time a score of minor errors (separately communicated).

STEPHEN HAY
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JOHN TREGENZA. *Professor of Democracy: The Life of Charles Henry Pearson, 1830-1894, Oxford Don and Australian Radical*. [Carlton:] Melbourne University Press; New York: Cambridge University Press, 1968. Pp. xv, 279. \$16.50.

FRANCIS WEST. *Hubert Murray: The Australian Pro-Consul*. New York: Oxford University Press, 1968. Pp. vii, 296. \$8.75.

The absence of biography as a major expression of Australian historical scholarship is gradually being corrected by the appearance of a number of fine studies in recent years. The two volumes under review are especially pleasing additions: well researched and attractively written, the two present the first scholarly studies of their subjects and go on to say something about personality and motivation in history. The one examines the life of one of the Anglo-Australians who helped to shape politics and to express an English liberal creed in Victoria in the 1870s and 1880s; the other assesses the life of Australia's first lieutenant-governor of Papua who became internationally known for

his "native policy" during his tenure of thirty-two years. Together the works demonstrate anew the strengths and weaknesses of biography—indeed, Francis West in a prologue to his study of Lieutenant-Governor Murray discusses the elements comprising good biography, and he vigorously applies his standards to Murray's entire career in New Guinea. Tregenza is less tense in dealing with his Victorian, but he skillfully shows how one man may influence his generation while still falling far short of his goals of personal success.

Both Pearson and Murray began their major careers after the age of forty, but the two had little else in common. Pearson was a more open personality, and he remained basically a teacher in all of his activities. Despite an element of drift in his early years and his limited success as a serious historian in England, Pearson seemed to find Australia suited to his needs and his solutions for society's ills. In Parliament for fourteen years and holder of the post of minister of public instruction for five, Pearson had an opportunity to apply some of his ideas, but as an active politician, Pearson was not necessarily at his best. His writings and his example of the intellectual identifying with the have-nots gave him a greater indirect role in Australian politics. Tregenza might have done more in tracing this indirect role and in placing Pearson's career within a broader Australian and Anglo-Australian context. (Exchanging the subtitle with the title or qualifying the title of the book would ensure the author of the readership he should have.)

Pearson at the close of his life brought out a book of prophecy that helped to reinforce Australian convictions to "stay white." Murray, an Australian of Irish ancestry, was part of New South Wales's elite, but until he slipped into his colonial career he was only a boxer and a Catholic of some note. Murray went to British New Guinea as chief judicial officer in 1904 and was later appointed governor when the territory became Australia's colony. He stressed then the need for colonization to benefit the white investors, but in his scale of values, Murray showed a tendency to seek an understanding of his Papuan subjects. In a chapter that shows the author's exceptional skill in probing Murray's responses, West tells how the judge

faced the more fundamental complexities of colonial rule. Subsequent chapters could have been strengthened by comparing Murray with those who were administering the other two parts of New Guinea and by dealing more fully with the governor's relations with Australian interests in the territory. That one man was allowed to rule for so long suggests not only that he had the time and the power to implement his ideas but that perhaps Papua did not fulfill the expectations held out by Australians at the turn of the century.

LESLIE CLEMENT DULY
University of Nebraska,
Lincoln

MARY SHANAHAN. *Out of Time, Out of Place: Henry Gregory and the Benedictine Order in Colonial Australia*. Canberra: Australian National University Press; distrib. by International Scholarly Book Services, Zion, Ill. 1970. Pp. xv, 187. \$6.65.

Mary Shanahan, principal of Duchesne College, University of Queensland, has written an excellent introduction to the history of the Roman Catholic Church in colonial Australia. Supporting her work with extensive reference to religious archives in Australia and England, she focuses on the work of the English Benedictine congregation in a skillful manner as she tells the story of the struggle between the congregation, "geared to meet needs of past times," and an essentially Irish constituency determined to make the Roman Catholic Church in Australia an instrument to serve the cause of Irish nationalism.

In 1835 the English Benedictine John Bede Polding was sent to Sydney as bishop to resolve differences that had arisen between Irish priests and English superiors in the Roman Catholic organization. From the Benedictine House at Downside, where Polding had been novice-master, he selected three young clerics to accompany him—among them was Henry Gregory, still a sub-deacon. For over a quarter of a century Gregory was to remain in Australia, where he founded a monastery and worked under Polding (raised to the rank of archbishop) as the vicar-general of the archdiocese of Sydney.

While Polding and many of the Irish priests in colonial Australia were poles apart in their outlook, he had no deep-seated dislike for the

Irish and seems to have done a reasonable job of governing the church in collaboration with them. Nevertheless, he felt that the church in a newly developing English colony was in need of broadening its structure. It was to help in the achievement of that goal that he made the inexperienced Gregory his vicar-general in 1843. An older man, even an Irish priest of wider experience would have been a better choice. The Irish were too independent, and Gregory and Polding failed to impregnate them successfully with the idea that Australian Catholicism should be "neither English nor Irish but Catholic."

In 1861 after many bitter struggles Henry Gregory was ordered home to England when papal authorities intervened in the hope of restoring peace to the troubled church in colonial Australia. The recall marked the end of an attempt to establish a culturally traditional, conservative, and authoritarian church, "out of time, out of place," in a land where the problems were largely practical and where, thereafter, rule was to be by an Irish hierarchy.

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ALASTAIR DAVIDSON. *The Communist Party of Australia: A Short History*. (Hoover Institution Studies, Number 26.) Stanford: Hoover Institution Press, Stanford University. 1969. Pp. xiv, 214. \$6.00.

This monograph, which is described as a short and pioneering history, is part of a series published by the Hoover Institution. The author, a member of the politics department in Monash University, bases his work mainly on party documents, government publications, and periodicals. He has done considerable interviewing among Australian communists and within the labor movement generally.

Dr. Davidson eschews both the wider theoretical implications of his subject and any detailed discussion of its social context. What emerges is an account of the fortunes of the Australian Communist party from its start in 1920 to the mid-1960s, with emphasis on the pre-1945 period. We are told that there has been much similarity between the experiences of the American and the Australian parties. So it would seem, for there are few surprises in this

narrative. When the Australian economy was in trouble and stories of worker triumphs in Russia came through, the local party did well. In boom times, however, or when communism was easily associated in the public mind with Russian egotism, the support of ordinary workmen was apt to fall off steeply. Despite the depression, communism did not prosper in the thirties, and Stalin's alliance with Hitler was no easier to explain in Sydney than it was in London or New York. Things improved during the war, but industrial expansion afterwards and Russia's bickering with her late allies had depressing effects on popular appeal. From a peak of 23,000 in the mid-forties membership dropped below 6,000 in 1957. By the sixties a mass movement seemed impossible. The intellectuals who stayed in the party agreed with union leaders that future aims should be expressed in traditional Australian workingmen's terms, no matter what Moscow wanted. The alternative, they thought, was extinction.

Dr. Davidson does say quite a lot, after all, about the society and the economy, and this helps counterbalance the effects of a somewhat pedestrian rendering and an annoying compartmentalizing of chronology, interpretation, and conclusions. But this is a workmanlike, well-documented, and useful tracing.

ROBERT HEUSSLER
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L. D. MEO. *Japan's Radio War on Australia, 1941-1945*. [Carlton:] Melbourne University Press; New York: Cambridge University Press. 1968. Pp. xv, 300. \$12.50.

BRUCE MUIRDEN. *The Puzzled Patriots: The Story of the Australia First Movement*. [Carlton:] Melbourne University Press; New York: Cambridge University Press. 1968. Pp. xiii, 200. \$13.50.

The only common theme in these two books is the Japanese threat to Australia in World War II. Mrs. L. D. Meo wrote the better book; she describes and analyzes Japanese radio propaganda beamed to Australia between 1941 and 1945. Bruce Muirden writes of the Australia First Movement and of the arrest and internment of the tiny central cadre in early 1942 for fear that they might help the Japanese invaders.

Mrs. Meo's study is a condensation of a 1951

master's thesis, based on the records of the Listening Post of the wartime Australian Department of Information's Short Wave Division on sources translated from Japanese. She neatly fits her theme into a clearly etched backdrop of Japanese internal politics. "In reality Japan was engaged in a triple war—war against the enemy, a bloodless war between the Army and the Navy, and internal political warfare." The opening chapters outline the bureaucracy for the dissemination of propaganda, part 2 shows how the propaganda worked out by the various groups was adapted to the segments of the Australian community, and the final section is an unusually perceptive analysis of the basis of Japanese propaganda toward Australia. During the first six months of victory it was fairly truthful; later it "sacrificed every pretense of credibility." Japanese censorship, coupled with inflexibility and ineptitude, hampered the propagandists. The use of clichés and catchcries, the refusal to face and to plan for change, and the extravagant claims as the Japanese position deteriorated are examined. Propaganda appealed to pride, vanity, prejudice, jealousy, and anger. Anti-Americanism, anti-Semitism, and war weariness were favorite themes. She analyzes devices called "word manipulation" and lists certain symbols, stereotypes, and slogans and proves that "some tens of thousands of Australian listeners" were deeply interested in the broadcasts because of messages from prisoners of war. There were approximately thirty thousand Australian POW's and internees in Japanese hands who had close relatives in Australia. It is amazing that by January 1945 the broadcasts were trying to lay the foundation for postwar trade between Japan and Australia. Mrs. Meo might have updated her bibliography more thoroughly.

Mr. Muirden is a skillful journalist with an eye for detail (sometimes excessive) and is the owner of a critical pen. However, there is not enough analysis and evaluation in his book. In March 1942 sixteen Australian-born men were secretly interned in Sydney on suspicion of helping Japan; some of them remained behind barbed wire until the war's end. The Australia First Movement was a small fringe group that was blatantly anti-British, anti-Semitic, pro-German, and pro-Japanese. Their leader was the Rhodes scholar and intellectual, P. R.

"Inky" Stephensen, who published the chauvinistic *Publicist* (1936-42), financed by the eccentric, wealthy, anti-Semitic W. J. Miles. Mr. Muirden proves that they all were odd, but asks if they were traitors. He probes the government's reasoning and methods in interning the group (including the more dangerous four in Perth) and describes the scandal when the news of the internment was published. Dr. Evatt, the federal attorney-general, was criticized, so he appointed an investigatory commission under Mr. Justice Clyde, which worked from June 1944 to May 1945 and cleared most of the internees. Mr. Muirden painfully probes what happens to civil liberties when regular legal processes are suspended because of security during an overwhelming crisis.

The Australia First Movement did not hold its first meeting until January 1942; it had about eighty members. The author makes several interesting points: (1) inaccurate evidence was used in the roundup, and there was no justification in saddling the Sydney group with the wild schemes of those from Perth; (2) Evatt virtually prejudged the Clyde Commission's work; (3) army intelligence looked bad in presenting evidence where "careful selections from correspondence" were used "in which the innocuous became the seditious"; (4) the Clyde report was debated inconclusively in federal Parliament in March 1946; (5) the commission findings (well analyzed by the author) showed a fuzzy disparity between the summary of facts and the recommendations, and too many unstated assumptions were used to provide the bridge in the arguments; and (6) Stephensen refrained from public comment until 1959 because he hoped for a further inquiry. The author concludes that the movement never contributed any great threat to Australian security.

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Irvine*

LIONEL WIGMORE. *Struggle for the Snowy: The Background of the Snowy Mountains Scheme*. New York: Oxford University Press. 1968. Pp. xx, 215. \$13.00.

This is an officially commissioned history of the beginnings of a gigantic water conserva-

tion, hydroelectric, and irrigation scheme in southeastern Australia. Started in 1949, the project involves diverting water from the Snowy River, which flows southward from the Dividing Range and Snowy Mountains to the Tasman Sea, into the Murrumbidgee-Murray river system, which flows westward from the other side of the ranges to Bass Strait in South Australia. Financed through loans from the Commonwealth government to be repaid from the sale of electricity, the undertaking is probably the largest multipurpose project ever carried out in the Southern Hemisphere and is comparable in many of its goals to the Tennessee Valley Authority. The engineering tasks have been of a much greater difficulty and magnitude. They have included driving long tunnels through the mountains, constructing huge storage and generating dams in remote areas, and building pumping stations so that water can be used many times over for the generation of electricity.

Lionel Wigmore deals with the background of the scheme rather than with its construction and operation. The first third of the book examines the geography, exploration, settlement, and early economic development (including irrigation systems) of the area. Two short chapters bring the study down to World War II. The balance of the survey summarizes the various proposals for the development of the Murray-Murrumbidgee basin and the Snowy River region. Of particular note are the problems of reconciling the conflicting interests of New South Wales, Victoria, and South Australia, and the need to reach constitutional compromises granting the national government authority to implement a regional development plan. An epilogue summarizes progress to 1968 and looks ahead briefly to the completion of the project.

The study is clear, interesting, and useful as far as it goes, but one wonders why author and publisher chose to limit coverage to the background phases. The reader is given little more than a prologue. Perhaps publication should have been delayed until the construction and operational phases of the scheme could be included. They are the elements of the story most worth telling.

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AMERICAS

VICTOR HOAR, compiler. *The Great Depression: Essays and Memoirs from Canada and the United States*. [Vancouver: Copp Clark Publishing Company.] 1969. Pp. vi, 232. \$3.75.

The nine essays included in this volume were originally read as contributions to a meeting of the Canadian Association for American Studies. Four of the essays are avowedly academic analyses of aspects of the depression years in the two countries while the other five are reflections by people who may best be called observer-participants in the decade. Only one of the authors sets out specifically to write "comparative" history, and perhaps this is a measure of the difficulties that surround any attempt to organize a comparative history conference. On the whole, however, the essays in each category meet a high standard, and thus the volume will prove stimulating to any student interested in the North American experience of the thirties.

In the academic group of essays James T. Patterson compares the depression impact upon the federal systems of Canada and the United States. Patterson's essay, while provocative, also points to the perils of comparative history. While he draws a number of convincing comparisons (between, for example, the political careers of Mackenzie King and F. D. Roosevelt) and argues that federal systems are inherently unable to cope with economic crises, he attributes Canadian-American reluctance to accept centralization to "an ethos of rugged individualism" that "pervaded both countries." Comparative history seems to induce this kind of contentious oversimplification, and, in the Canadian-American case, it leads also to exaggeration both of similarities and of differences.

William E. Leuchtenburg's essay on "The Constitutional Revolution of 1937" is a straightforward review of the New Deal-Supreme Court struggle in which he concludes that pro-Hughes analysts of this topic are wrong when they argue that the Court's switch-in-time was simply a reassertion of previous principles ignored during the period of judicial aberration. H. Blair Neatby contributes a brilliant essay describing the thinking and methods of Mackenzie King as that astute political broker dealt with the threats to his

power posed by the regional and class discontents of the depression years. In his essay "Marxism and American Literature" Kenneth Ledbetter gives a fresh and interesting analysis of the role of Marxism in American writing of the thirties. Ledbetter argues that nearly "every American writer worth his salt" had some kind of love affair with the Communist party in those years and that in nearly every case the affection dissolved in the conviction of vicious and cynical betrayal.

The five personal memoirs of the thirties by Hugh MacLennan, Saul Wellman, F. R. Scott, Hazen Sise, and Graham Spry bring to bear a good deal of recollection, both general and specific and for the most part closely related to the academic themes. In this second category Scott comes closest to the comparative approach when he argues that the creation of new parties in Canada illustrates "the greater flexibility to be found in the parliamentary by comparison with the congressional type of constitution" and that this flexibility accounts for the significantly lesser attraction of the Communist party for Canadian intellectuals and left-wingers than was the case in the United States.

KENNETH MCNAUGHT
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ROWLAND BERTHOFF. *An Unsettled People: Social Order and Disorder in American History*. New York: Harper and Row. 1971. Pp. xvi, 528. \$12.50.

In 1960 Mr. Berthoff published in this journal an article of striking originality and reverberating importance. (Rowland Berthoff, "The American Social Order: A Conservative Hypothesis," *AHR*, 65 [1959-60]: 495-514.) He began by repeating Samuel Eliot Morison's appeal of ten years earlier for a new conservative synthesis of American history. He mildly deplored undue concentration on the history of ideas and called for a new kind of social history, which would abandon random compilation and concentrate on "the structure of society—and the functional interplay of the various institutions and population groups that make it up." What made Mr. Berthoff's article important was not these worthy but then fairly standard exhortations but rather his suggestion of a new periodization, one that was highly interesting in itself

and went some way to define the kind of conservative social history he had in mind.

The first phase of American history, he said, ran from the foundings to 1815, and saw the establishment of a "fairly stable" social order. The second phase, the nineteenth century, was "an epoch of enormous migration, immigration, and social mobility, during which the recently established social order became badly disorganized and in fact disorderly." Next came "a transitional period from about 1900 to 1930, during which free land and free immigration came to their end and a profound effort to reorganize American society began." Finally, since 1930, Americans had established "a society which, although still highly mobile, is better integrated than that of the nineteenth century and is in this respect more comparable to that of the eighteenth."

Professor Berthoff's present book is an attempt to put some flesh on this arresting skeleton. It has been eagerly awaited by those who admired his article. If carried out successfully, this reinterpretation would reverse the standard value system of American historians far more drastically than the work of any New Left historian, though in a way not irrelevant to some New Left criticisms. Mobility and the frontier become the villains of the traditional drama; stable institutions, even stratification, the heroes. Of course such suggestions have been made before by Americans, but not by many recent historians. The 1960 article made it clear that Professor Berthoff's vision owed something to two related tendencies of the 1950s, the growth of neo-conservatism and the flourishing of historical sociology. One could not but wonder how his thesis had survived the apparent decline of both, and whether, in 1971, the main current movement seemed still to be in the direction of reintegration and eighteenth-century order.

It is with real regret that I must report that the book, though interesting and suggestive at many points, does not succeed in bringing off its great enterprise, a persuasive reorganization of the whole of American history. Its failure, insofar as it is a failure, arises from two causes. The first is that Mr. Berthoff does not seem to have re-examined his thesis closely and deeply enough in the light of new ideas, information, and events. By this I do not mean that histori-

ans should always be up to date in their interpretations. Fashionable reversals of opinion are one of the occupational diseases of American historians. Professor Berthoff suggests in his introduction that he may be undertaking at least a partial defense of what has been called "consensus history." Well and good; there are things well worth defending in the work of historians so classified. But a defense must take account of attacks, and a new conservatism must be, if not entirely new, at least freshly re-examined. There is too much reiteration here and not enough development. The second main deficiency of the book arises from its method and use of sources. Mr. Berthoff's social history, as here presented, is much more like the old miscellany than the new concentrated analysis he called for.

The main way in which the book seems to show a lack of sharp rethinking is in the restated periodization. This is changed in detail, but still suggests a cycle from integration to disintegration to reintegration, and it is this cycle that seems to me not to be convincing. In the first period, which now ends in 1775, "a viable branch of the familiar English social order" was established in America. The church, the family, and recognition of rank and degree remained fairly strong, and so did the idea of an organic commonwealth. All these were beset by forces of change, but the lack of population and other brakes on economic development "kept the old European social values remarkably safe from radical upheaval."

Though not all the exponents of the new colonial social history would support this characterization, it seems possible to look at the colonial period this way, as a time of relative stability and thwarted movements of change. A historian is free to emphasize either innovation in relation to Europe or continuity in comparison to the later United States. At only one point in this part does Mr. Berthoff's organization really creak badly. The Great Awakening, which in his scheme was an anarchic and disorganizing development, is simply moved to the next section and discussed as the beginning of *nineteenth-century* religious disorganization. It was too big, and came far too early, to be treated so perfunctorily.

The second period, that of individualist dis-

organization, now runs from 1775 to 1875. Mr. Berthoff's version of this period seems reasonably convincing and indeed familiar. He is simply standing on their heads those historians, from Parton on, who delight in Jacksonian victories against corporations, banks, courts, and Philadelphia stuffed shirts. The major destructive forces at work were economic development, internal migration, and (after 1815) immigration. Together these broke down the promising conservative order of the previous periods. Family, church, and community lost solidity and influence, and the shreds of the commonwealth idea were abandoned in favor of the dogma of the unlimited right of private property. Conservative reactions to this disorganization—and here Mr. Berthoff seems more original—including revivalism, reform movements, voluntary organizations, and popular education failed to reverse the basic disorganizing tendency and only made things worse. The reason for this failure was that the backers of these counter-movements believed that freedom from institutional constraints would cure society's ills, whereas really such freedom, or rather disorganization, was the source of the trouble. The Civil War was the culmination of this long period of disintegration, and its outcome—despite the mystical idea of union—increased the social confusion. As Mr. Berthoff sees it, there was more than one blundering generation. My only quarrel with this presentation of this period is a mild one, that Mr. Berthoff somewhat underrates the persistence, real though limited in effect, of countervailing forces: the survival of an Eastern urban elite, the conservative moral and social dogmas universally inculcated in schools and colleges, the considerable surviving influence of the churches despite (or perhaps because of) their compromises with the spirit of the age.

The title of the third section is "A Reconstituted Society 1875-1945." It is here that Mr. Berthoff seems hardest to follow, since most of his narrative shows that very little *social* reorganization took place. The economy was indeed reorganized, first by large enterprisers, then by essentially conservative Progressives, finally by the equally conservative New Deal (here Mr. Berthoff clearly comes out in the same place as some Left historians). Yet, he tells us again and again, all this achievement of reorganization

was vitiated by failure to transcend the business values of production and acquisition. As a result, "the American social structure continued, in a still materialistic age, to be a more or less fortuitous by-product of economic processes." With the exception of mass immigration, halted in the twenties, all the forces of disintegration speeded up. Moreover, disorganization, accidental in origin, was increasingly sanctified by the new liberalism with its catchwords of "personal freedom." Indians, blacks, and new immigrants were the victims of delusive dogmas of equality of opportunity. The family became companionate, the churches destroyed their remaining authority by accommodating completely to social pressures. Even the immigrant slums, which in their way served an integrative purpose, were swallowed in the impersonal order of city, factory, and suburb. Even more than earlier, conservative efforts from comstockery to the Ku Klux Klan failed completely to aim at the right targets. The public schools, which Mr. Berthoff correctly sees as the major instrument of social indoctrination, did not steadily pursue the right objective. Mr. Berthoff sees much more hope in the development of a closer network of voluntary organizations of all sorts, and even suggests toward the end that in these a possible framework for social organization was at hand by 1945. Yet the overwhelming conclusion of his study must be that Americans had so far failed to make much use of them for this purpose.

The book ends with a tantalizing postlude of twenty-two pages on the period since 1945. Mr. Berthoff sees some return to the commonwealth idea in the Republican acceptance of economic controls (it is too bad the Nixon price-wage freeze came after publication). He suggests that the concept of community, which he does not restate in contemporary terms, was rediscovered in the sixties, but that a "workable structure" for it was hard to find. Such social crystallization as took place was likely to be denounced by liberals as the work of organization men or the power elite. One wishes that this last section of the book had been more developed. If it had been, however, and if it had not stopped in about 1963, it would have had to find a place for many kinds of rebels and segregationists, hippies and dropouts. To my

mind these taken together amount to a phenomenon that has to be taken much more seriously than is suggested by Mr. Berthoff's very brief references to echoes of the disorderly past.

Thus the movement from integration to disorder and back to integration, announced in the article and from time to time reiterated in the book, seems to me to prove illusory. If Mr. Berthoff were to abandon it, his interpretive reorganization might be more effective. One might be able to see more clearly in each period the dominant tendency toward social disorganization struggling with sizable, though unsuccessful integrative counterefforts. Such a dichotomy cannot of course organize or explain everything, but I see no reason why it should not prove just as effective as others that have served us well in their day—industry versus agriculture, privilege versus equality, and the like.

The second main defect of the book is one of method and sources, about which Mr. Berthoff is admirably honest. He tells us in the introduction that he is hampered by the failure of American historians to write monographs on the subjects most relevant to his needs. Hence some sections of the book had to be hammered together "out of whatever evidence comes to hand." Too much of Mr. Berthoff's narrative, as his citations show, is drawn from standard secondary works by well-known historians. Well-worked theses, from the safety valve to the status revolution, are worked awkwardly into his own new organization. The sharp impressions one can get only by asking the sources one's own questions are lacking. Jefferson is quoted from a book by Mr. Sydnor, Niebuhr from Mr. Carter, Webster from Mr. Van Deusen. It is instructive to see how the book comes to life in the passages where Mr. Berthoff is closer to his own research, especially in the sections on immigration. It would have been better to have had more of this, and to have left out the fall of the Virginia Company, the Philadelphia and New York prison reform systems, the spread of the cotton kingdom, and many other textbook topics that are treated too briefly and conventionally to make them serve Mr. Berthoff's purpose.

If this review is too severe—and I think it may well be—it is because respect for Mr. Ber-

thoff's intelligence and fascination with his thesis has led to too high hopes. He has failed, as anybody well might, to present the whole of American history successfully in terms of a new interpretive structure. I hope very much that, having had a good try at this noble enterprise, he will sharpen his tools for a less ambitious effort. He could, for instance, provide us with a history of the organic, conservative, integrative minority movement in all periods with more analysis of its ideas. He might give us a detailed look at the fifties, a crucial decade at least for his scheme, and tell us how seriously we should now take the conservative tendencies in philosophy and religion, popular and high-brow literature, social science, and history that seemed so strong at that time. Perhaps, seen properly, these really were the wave of the future and the present ultra-libertarianism simply the last futile protest against them. Perhaps.

To write either of these needed books, or others that he can suggest much better than a reviewer can, he would need to tell us a little more clearly just what he means by reintegration. What are the higher noneconomic values he often invokes, and whence are they to be drawn? Was there an alternative course for American society entering the industrial era; did any other society find one? Mr. Berthoff is uniquely qualified to meditate about these matters. One of his best qualifications is that he is clearly not a backlash historian; his conservatism has no place for futile resentment.

It may be, on the other hand, that part of the interpretive trouble comes from Mr. Berthoff's obvious humanism and humanitarianism. He seems unready completely to repudiate the choices that have been made for mobility and equality as against stability and hierarchy. The fundamental misconception of the nineteenth century, he tells us, was "that an egalitarian society necessitated an unregulated economy." Can he envisage the development of regulated equality? Regulated by whom? Doesn't a society, or a historian, have to make harder choices than this suggests? There was never a better time than the present for asking such truly subversive questions about the American past.

With all its faults, I hope that this book will

be read and pondered, and still more that further efforts will be made, by Mr. Berthoff and others, to explore the deeply counterrevolutionary thesis proposed in the 1960 article.

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Cambridge

DARRETT B. RUTMAN. *American Puritanism: Faith and Practice*. (Pilotbooks: The Lippincott History Series.) Philadelphia: J. B. Lippincott Company. 1970. Pp. xii, 139. Cloth \$4.95, paper \$2.25.

TIZIANO BONAZZI. *Il sacro esperimento: Teologia e politica nell'America puritana*. (Saggi, Number 93.) [Bologna:] Il Mulino. 1970. Pp. 515. L. 6,000.

Both of these books attempt to correct Perry Miller's formulation of our understanding of American Puritanism, but they have little else in common. Rutman seeks to define Puritanism within a conceptual scheme that will help connect an older school of thought (Miller's) devoted to studying New England as a Puritan idea and a newer one devoted to studying it as a society. Bonazzi analyzes the political application of covenant theology in Massachusetts from a wide perspective in order to assess the significance of the holy experiment in modern history.

Defining Puritanism (with Haller) as a Christian fellowship of ministers that emerged in late sixteenth- and early seventeenth-century England, Rutman sees the brotherhood's evangelical zeal imparting to laymen a twofold gift. One was an exaggerated traditionalism. The preachers identified God with entrenched social values at a time of rapid change, and listeners who participated in the Great Migration understandably made New England a traditional society, as recent demographic and social historians demonstrate. The other gift was a quickened religiosity. The author interprets individual conversions psychosocially as the resolution of emigration-induced identity crises in terms of the available ideology. The New England clergy, torn between their Puritan zeal to convert souls and their ministerial desire to build the church, temporarily reconciled these incompatible commitments by equating the covenant of grace with the church covenant.

But after the ministerial commitment triumphed in the Antinomian crisis, the church covenant became more important than individual regeneration. Then, ironically, the Puritan churches no longer exercised an evangelical function, and New England possessed only the semblance of unity.

Rutman argues his case clearly and provides some suggestive insights as well as a brief but valuable bibliography. Yet his portrait is unbalanced. He ignores the role of the state as a guide to the church and treats Puritan ideas (covenant theology) at best casually. His attempted synthesis remains unconvincing.

Bonazzi regards Puritan Massachusetts as the best exemplar of the practical attempt, inspired by the Reformation, to reconcile the world with the spirit. He starts by tracing the origins of covenant theology, which is central to his analysis, back to the great Reformers. Luther depreciated reason and insisted that salvation depended not on good works but on divine love, thus denying man all worldly initiative. Calvin, on the other hand, restored reason to the cosmic scheme and emphasized activism as a means of glorifying God. In Puritan federal theology a covenant bridged the abyss separating creature and Creator. Reason was primarily a means of understanding God's revealed will, although it also enabled the elect to participate in their own salvation. The doctrine of vocation taught the saint to find his proper place in a providentially ordered creation, not in solitude but in concert with the people of God. Thus covenant theology made the subjective experience of regeneration the objective standard for constructing a Christian commonwealth.

The second part of the volume describes how Massachusetts Puritans used covenant theology as a means of social organization. They viewed themselves as the new people of God called to execute the divine will by effecting a social revolution in the promised land. As Winthrop declared, a bond of love united the whole community in covenant with God and constituted the only valid principle for building a Christian society. To realize this ideal the Massachusetts Bay Company violated its charter and put the government in the hands of saints whose personal vocation enabled them to govern the New Israel in accordance with its divine voca-

tion. But this attempt to regenerate the world failed. The freemen insisted on limiting civil authority in accordance with the charter and English rights, and a series of struggles— notable examples are the restoration of legislative power to the General Court in 1634 and the establishment of bicameralism in 1644— finally demonstrated that power mandated by the political process rather than Christian charity preserved the unity of the commonwealth. In addition, Massachusetts became repressive, binding men externally through the law rather than internally through the conscience.

This brings the author to the Antinomian controversy. He contends that Cotton and Wheelwright remained closer to the original covenant theology than their orthodox antagonists in treating the role of grace and reason in the work of regeneration. Since the theological conflict between doctrines of inner grace and visible sanctification had political ramifications, civil authorities intervened to impose an external behavior conformable to practical needs, and the state designed Anne Hutchinson's trial to sanction the ritual killing of an enemy. Thus the holy experiment persuaded itself of its continued adherence to its spiritual mission. In the Antinomian crisis the only violence against individuals was psychological and spiritual, but the use of intellectual means to coerce a whole commonwealth revealed cultural totalitarianism.

An epilogue emphasizes the significance of the new mode of looking at society discovered by Massachusetts Puritans. They made of creation an intelligible machine that worked for God's glory and in ways comprehensible to man, but they understood the saint as a gear in that machine. Puritan rationalism opened the way to a secular understanding of the universe, but it was *not* the liberating force in American culture that Perry Miller made of it. Moreover, when political theology failed to give life to spiritual regeneration in Massachusetts, the state filled the vacuum by stifling criticism and determining the conduct of its members. Thus "the holy experiment laid the foundations of one of the most ambiguous pretensions of the modern world—that of seeing in the rational (scientific) organization of individual and social acts the answer to the ethical problem of how man might act humanly" (p. 464).

Bonazzi's volume, which will undoubtedly appeal to the New Left, is not without shortcomings. It lacks a bibliography, its citations are too often inaccurate, and other minor errors are unsettling. The discussion of Luther and Calvin slights their differences over the relationship between the two Testaments (the law and the Gospel), and the author has not always consulted recent authorities on critical aspects of his subject (e.g., Norman Pettit on preparation for salvation, David D. Hall on the Antinomian controversy). Yet this is a work of broad and sustained scholarship. Using Massachusetts as a laboratory for studying the relations between man and God, Bonazzi, a young American history professor at the University of Bologna, has drawn significant and provocative conclusions. To me his thesis is overstated. Rutman and Bonazzi should be read as correctives to each other. Together these books point the need for further work on American Puritanism.

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HAROLD L. PETERSON. *Americans at Home: From the Colonists to the Late Victorians. A Pictorial Source Book of American Domestic Interiors with an Appendix on Inns and Taverns*. New York: Charles Scribner's Sons. 1971. Pp. xviii. 205 plates. \$14.95.

Pictures are social documents, as Harold Peterson's source book of American interiors clearly proves. Believing that a house and its contents "both reflect and condition attitudes, impose limitations and provide the environment for achievement," and that, therefore, a "basic knowledge of the physical surroundings that comprised the American home and a sense of their significance" are essential to an understanding of American social history, Peterson selected pictures indicating the use of objects or room arrangements between 1659 and 1876 to this end. The paintings, engravings, and contemporary photographs collected here, although badly reproduced, are of interest in this respect and ought to provide social and cultural historians with some valuable insights.

Unfortunately, Peterson's text does not provide the interpretation he promises or even the clues that would guide the historian, untrained

in the use of visual materials, into an understanding of the relationship between specific objects and furnishings and social needs, economic development, or cultural tastes. What does it mean, for instance, that the first waste-paper basket did not make its appearance in a home until the middle of the nineteenth century, that the dahlia was more popular than the rose, or that slipcovers covered the wood of sofa legs without the use of a skirt? Obviously, the presence or lack of decorative objects that have today become common household furnishings can tell us something about the society that created them or omitted them, highlighting the sparse life of the frontier, the degrading environment of lower-class tavern patrons, the efforts toward gentility of a middle-class people with limited income but high social aspirations. Peterson's book would have been far more readable as well as useful had he shared some of his thoughts about these matters with his readers, rather than describing perfunctorily what the eye sees.

Because he does not explain or interpret and because he is not concerned with esthetic matters, Peterson's book is limited in its usefulness. As a source book, however, it serves a function. Many scholarly and beautiful books exist dealing with fine American furniture and with upper-class furnishings that have been deposited in the permanent collections of Winterthur and other art museums; the furniture pictured in this volume, with its essentially middle-class orientation and lack of fine craftsmanship, was more readily discarded and, therefore, with the alternations of fashion has disappeared. Since our only knowledge of many of these objects must come from such painted or photographed illustrations as Mr. Peterson includes here, we can only be grateful to him for collecting such a vast number of them and making them available to us in this useful collection.

LILLIAN B. MILLER
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 Smithsonian Institution*

ROBERT A. RUTLAND, editor. *The Papers of George Mason, 1725-1792*. Volume 1, 1749-1778; Volume 2, 1779-1786; Volume 3, 1787-1792. (Sponsored by the National Society of the Colonial Dames of America, the Board of Regents of Gunston Hall, and the Institute of Early American History and Culture.)

Chapel Hill: University of North Carolina Press. 1970. Pp. cxxvii, 483; xxi, 487-864; xxviii, 867-1312. \$45.00 the set.

George Mason's papers were far from complete when Kate Mason Rowland published her life-and-letters biography in 1892. Tragic gaps remain, but many additional documents have been ferreted out by Dr. Rutland, who has arranged and edited all of Mason's public and private papers, plus correspondence addressed to this Virginia planter. Even missing letters are cataloged and commented upon whenever possible, and over one hundred speeches delivered by Mason at the Constitutional Convention in Philadelphia are also included. The first volume contains both a provocative introduction and a handy glossary of names and places. Altogether this is a stellar performance, worthy of the editor's announced design to "reintroduce" George Mason to American history.

To be sure, American historians had never really forgotten Colonel George Mason, the statesman who wrote the Virginia Declaration of Rights and opposed the federal Constitution. Perhaps the public at large demands more personal flair in its enduring heroes, for life at Gunston Hall in Virginia's Fairfax County was a serious business. There is wisdom on display in these volumes, but if the wit of this flinty tobacco entrepreneur were extracted, it might not fill a single page. On one memorable occasion, Mason tried very hard to make a joke about marriage, a standard target. Probably to his subsequent regret, the remarks came out sounding crude. Miss Rowland, the first editor of a selection from these papers, found it unnecessary to snip out much that would offend the Victorian sensibility.

Dr. Rutland has followed the already classic editorial guidelines established by Julian P. Boyd in *The Papers of Thomas Jefferson*. In fact, the notes after each document too frequently hold the reader's attention better than Mason's text. That may be because the editor has made a deliberate decision to include a certain amount of "informed speculation." Only rarely does Dr. Rutland skate on thin ice when discussing the precise source of an idea. For example, the notion that a country might be punished for its collective sins, in this case the keeping of slaves, may have come from Mason's

Bible instead of from Thomas Jefferson's *Notes on the State of Virginia* or from enlightened contemporary opinion.

The editor sees George Mason as an honorable man of bold principles who occasionally rose to the level of political philosopher. It may be recalled that in an earlier short biography Dr. Rutland labeled the master of Gunston Hall a "reluctant statesman," for Mason lavished his primary allegiance on his land and his family. In the entertaining introduction, Colonel Mason is further characterized as "a valetudinarian of the first rank." The truth of this observation is manifest. Mason's papers show that he regularly excused himself from undertaking public duties or for tardiness in answering his numerous correspondents with a clinical exposition on his health. At least once he even pleaded disability by reason of "a very sore Finger," which had prevented him from holding a pen.

Mason's curse was gout, but he was surprisingly energetic. Better-known contemporaries such as Washington, Jefferson, Madison, and Richard Henry Lee admired and consulted him. He was also highly acquisitive, even in his personal dealings. Following the death of his beloved wife, Ann Eilbeck Mason in 1773, he wrote a will of monumental proportions. He was never compelled to alter this cumbersome document, for when he remarried in 1780 he sealed the vows with a legal agreement that made all the necessary allowances for his wife, and any "Issue of the said Marriage." For better or for worse, the editor has accepted the judgment of Miss Rowland, who established the age of Mason's bride, Sarah Brent, at about fifty.

Perhaps there is a lesson here. George Mason was always careful to provide as best he could for any eventuality; by nature he was a "man of little faith." As treasurer of the Ohio Company, his lifelong struggle to prop up that unfortunate organization's enormous land claims in the West taught him not to trust in charters or promises. Mason's justly famous Virginia Declaration of Rights—whose memorable phraseology found its way into the Declaration of Independence—was "insurance" for the people of his colony. Together with Thomas Jefferson, he converted the promises contained in the Declaration of Rights into a constitution

for the commonwealth of Virginia. And in 1787 Mason left his retirement to attend the federal Convention at Philadelphia, where he fought for his principles and refused to sign the Constitution because it did not contain all the guarantees he wanted, including a Bill of Rights. As an Antifederalist at the ratifying convention in Virginia, Mason engaged in a losing battle for the inclusion of clauses that, to his mind, would forever preclude "a monarchy, or a corrupt, tyrannical aristocracy."

As Dr. Rutland notes, George Mason was almost unique in that he disdained personal power. He could have accepted the federal Constitution and probably capped his public career with a prestigious office in the new government. In his twilight years he would have then avoided making enemies, including his old friend and close neighbor, George Washington. He must have been sorely tempted to make his peace with Congress by accepting a seat in the Senate when it was offered in 1790. That he did not do all these things is tribute enough to a man with enduring principles and reason enough why historians of the colonial era will welcome these three excellent volumes.

A. R. RIGGS

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DAVID J. ROTHMAN. *The Discovery of the Asylum: Social Order and Disorder in the New Republic*. Boston: Little, Brown and Company. 1971. Pp. xx, 376. \$12.50.

NORMAN DAIN. *Disordered Minds: The First Century of Eastern State Hospital in Williamsburg, Virginia, 1766-1866*. (Williamsburg in America Series, Number 8.) Williamsburg: Colonial Williamsburg Foundation; distrib. by the University Press of Virginia, Charlottesville. 1971. Pp. xiii, 207. \$5.95.

EDWARD JARVIS. *Insanity and Idiocy in Massachusetts: Report of the Commission on Lunacy, 1855*. With a critical introduction by GERALD N. GROB. (Commonwealth Fund Book.) Cambridge, Mass.: Harvard University Press. 1971. Pp. 71, x, 9-213, 15. \$9.00.

"Institutions, whether social, political, or economic," David J. Rothman observes in introducing his account of the establishment between 1820 and 1850 of penitentiaries, insane asylums, reformatories, orphanages, workhouses, and almshouses, "cannot be understood apart from the society in which they flour-

ished." Now that the social impetus behind Jacksonian politics and economics—the development of parties, voting behavior, banks, antebellum slavery, industrial classes—is becoming familiar, a social interpretation of institutions that wrestled with indubitably social problems can hardly fail to enhance our understanding of that era. Professor Rothman's introductory sketch of colonial society as a hierarchic and communal structure in which even delinquents had their accepted place begins well.

But once past 1820, the spread of social disorder, apparently defying direct analysis, leads Professor Rothman to fall back on the narrower matter of "Americans' understanding of these changes." This in turn boils down to the social diagnoses of wardens, commissioners, superintendents, chaplains, and other "students of deviant behavior," even though the public found some of their prescriptions—against outdoor poor relief, for instance—too stringent to apply consistently. It follows from this somewhat tautological method (explanatory context and subject becoming one and the same) that Professor Rothman declines to consider whether anything in the reformers' own social status or cultural inheritance may have imparted a distinctive thrust to their thought. Calvinists, Unitarians, Quakers, old elite, or new professional men—here all "Jacksonian" opinions are equal.

If the asylum ideology was indeed so widely shared, the outward ebullience for which the era is usually remembered was a mask for an "almost hysterical sense of peril" over the leveling of social ranks and the collapse of "stability and cohesion." The only optimistic note was the vague faith in "moral reform," the hope that even murderers and lunatics might be turned into responsible citizens if surrounded by proper influences. Marvin Meyers and others have convincingly shown that there was an undercurrent of anxiety that accounts for many otherwise paradoxical phenomena of the time. But that the vaunted liberty and equality of the new republic threatened general social collapse—something far worse, that is, than the sum of the pecuniary temptations, political excitements, religious enthusiasm, and parental laxness that this or that group of reformers blamed for crime, insanity, or pauperism—

seems to have been discerned by only a few unusually astute critics.

In any event, the ideology of the asylum was hardly as novel or as indigenous as the thesis of "discovery" suggests. In the absence of contrary evidence, we may continue to suppose that European thought and example contributed more than "convenient and impressive footnotes" to the moral reformers' contemplation of Jacksonian society. It seems odd in a book dedicated to Oscar Handlin, moreover, to find the asylums so absolutely dissociated, as organized institutions, from the commonwealth of the preceding age. Like canals and other public works in this era of the not yet wholly private business corporation, the asylum, though novel in form, might be considered to belong to the twilight of the old social order at least as much as to the dawn of disorderly individualism. The reformers still assumed that everyone *ought* to have his place in society, even though the old hierarchy of accepted places had been swept away. As Professor Rothman observes, the asylum was designed to teach obedience and respect for authority as the family no longer did—in particular the negligent or over-indulgent family of the incipient criminal, lunatic, or pauper. And yet he implies that even the old-fashioned family was not much of an institution by comparison with one of those fortress-like buildings where the unfortunates of the nineteenth century were, in the vernacular, "institutionalized." Only this colloquialism gives any meaning to the assertion that colonial Americans were "eager to avoid institutional solutions" when they farmed orphans and paupers out to local families, or that the twentieth century has once more been "gradually escaping from institutional responses." Jacksonian society was not peculiarly institutional; it was peculiarly balanced between hopefulness about individual perfectibility, preferably free from institutional constraints, and a lingering sense that constraint was still necessary.

The balance did not last. Between 1850 and 1870, as society at large slid into untrammelled individualism, coercion became the special fate of those without a place in it. The massive prisons and asylums thenceforth served mainly to ensure the safety and property of respectably independent citizens by locking criminals, mad-

men, or the undeserving poor away at minimal expense. Moral reform had been overwhelmed by growing numbers of long-term convicts, chronic mental cases, and hopelessly demoralized immigrants, which obviated individual treatment of any kind. But it had always suffered from the incongruity, which Professor Rothman voluminously documents but does not identify as such, of attempting "to secure social stability through individual rehabilitation." It was doubly futile to train an inmate to cope with the actual instability of "the open, free-wheeling, and disordered life" of that era by imposing on him a regimen that one orphanage keeper described as "*systematic labor of order and regularity established, and discipline enforced.*" In the extreme case of the penitentiary, individualism and constraint became one. The convict, "buried from the world" in solitary silence for a fixed term of years so that he might, as "the instrument of his own punishment, . . . listen to the reproaches of conscience," was exquisitely racked by what Tocqueville called "the most complete despotism" to be found in this land of "the most extended liberty." The asylum did not truly break with the past until it settled for keeping order among its mainly foreign-born and lower-class inmates and left moral self-help to more fortunate individuals outside the walls.

From this ambitious, suggestive, but somewhat tendentious study one turns the more appreciatively to the modest clarity of Professors Dain and Grob. In a sequel to his *Concepts of Insanity* (1964), Norman Dain traces the rise and fall of moral reform in the first, though not one of the leading, state institutions. Conversely, Gerald N. Grob follows his book on a model asylum, *The State and the Mentally Ill* (1966), by a general essay placing the influential Jarvis report of 1855, and Dr. Jarvis himself, within the shifting social and intellectual context of its time. Both accounts come closer than Professor Rothman's to the goal of relating institutions to their social setting.

ROWLAND BERTHOFF

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HENRI DESROCHE. *The American Shakers: From Neo-Christianity to Presocialism*. Translated from the French and edited by JOHN K. SAVA-

COOL. Amherst: University of Massachusetts Press. 1971. Pp. 357. \$9.50.

Desroche, a French sociologist of religion, has employed Troeltsch's typology of religious forms and Marxist materialism to produce what is certainly the most stimulating and provocative analysis of Shakerism yet to appear. His stated purpose is not to add any new information to Shaker history. His reading was restricted to the extensive printed sources and other materials available in French and British libraries, including heavy reliance on Daryl Chase's unpublished University of Chicago dissertation of 1938; nor is there evidence that he has visited the Shaker sites or has much feeling for the American historical scene. Nevertheless, his skillful use of analytical tools and especially his close reading of the Shaker texts has resulted in the best available account of the mind and institutional life of the Shakers.

Desroche follows the Marxist tradition in identifying Shakerism as a latter-day manifestation of Christian sectarianism that at the same time displayed "the primitive social consciousness of a British proletariat still in its Luddite phase." Newly arrived in America, the Shaker immigrants also displayed the common tendency of settlers to revert momentarily to an unpremeditated communitarianism. (Although Desroche declares his intention to use Bestor's terminology, the key term "communitarianism" does not survive the translation.) The peculiar brand of Shaker millenarianism and perfectionism isolated the believers from the surrounding American society and prompted them to make permanent the communitarian institutions that other settlers quickly abandoned. By the latter part of the nineteenth century, the self-sufficient Shaker agriculture and handicrafts were rendered obsolete by large-scale industry.

Much of Shaker history seeps through the coarse mesh of this interpretation. Ann Lee's origins as a Manchester factory girl may indeed explain her subsequent career. But nine immigrants fleeing from the harsh conditions of early English industrialism can hardly explain the conversion of thousands of native Americans to Shakerism, most of whom seem to have been property-owning farmers. Desroche says nothing about the socioeconomic circumstances of these converts or of their reasons for joining

beyond a casual reference to the "parasitic" relationship of Shakerism to evangelical revivalism. The documents clearly suggest religious motivations that Desroche recognizes but does not pursue. He plays down the theological milieu in which the Shaker settlements emerged because he regards doctrines as post facto rationalizations. This probably accounts for his lack of interest in the peculiar type of millenarianism and perfectionism prevalent among the Protestant revivalists from whom the Shakers were recruited. He is content to view Shaker communitarianism as part of the "ritual response" of religious immigrants to American conditions, resulting in such communities as Harmony, Zoar, Bishop Hill, and Amana, not realizing that these responses were the exception rather than the rule. The peak of Shaker recruitment was reached by 1830, well before the decisive impact of industrialism on American agriculture.

The strength of the book lies in its account of Shaker doctrines and organization, and in its comparisons of Shakerism with other sectarian movements. Its weakness is its failure to show how Shakerism was a product of the American scene.

STOW PERSONS

University of Iowa

CHARLES M. WILTSE, editor. *The Papers of Daniel Webster on Microfilm*. Ann Arbor: University Microfilms, in collaboration with Dartmouth College Library, Hanover, N. H. 1971. 41 reels. \$550.00.

CHARLES M. WILTSE, editor. *Guide and Index to the Microfilm*. (Microfilm Edition of the Papers of Daniel Webster.) Ann Arbor: University Microfilms, in collaboration with Dartmouth College Library, Hanover, N. H. 1971. Pp. 175. \$5.50.

Daniel Webster had a deep awareness of his history and a lively concern for his own place in it, yet gave little or no thought to the preservation of his personal records—unlike some of his contemporaries and quite unlike recent politicians, especially those who reached the office to which he aspired in vain and who left presidential libraries as a latter-day equivalent of mausoleums or pyramids. As a result of Webster's carelessness, his papers were widely scattered and their publication in a definitive form

was delayed in comparison with the writings of other men of his time, such as John Quincy Adams, John C. Calhoun, and Henry Clay. Not until about five years ago did Dartmouth College, in association with the National Historical Publications Commission and University Microfilms, undertake the task of searching out, bringing together, and reproducing the Webster papers as a whole. As its first fruits the project, under the general editorship of Charles M. Wiltse, has now yielded forty-one microfilm reels and a printed *Guide and Index to the Microfilm*.

Reproduced on the film are not only the documents in the main Webster collections at Dartmouth College, the New Hampshire Historical Society, the Massachusetts Historical Society, the Library of Congress, and elsewhere, but also more than 16,000 forgotten items that the editors managed to track down in the possession of some three hundred institutions and individuals. Reel 1 contains alphabetical card catalog entries for the entire set; reels 2 through 28, general correspondence; reel 29, business papers; reels 30 through 37, congressional documents; and reels 38 through 41, State Department records. Omitted are the State Department's official files during the two periods when Webster served as secretary. Also excluded are the reports of cases that he argued before federal, state, and county courts. The State Department files, the Supreme Court records, and many of the lower federal court records are already available on microfilm from the National Archives. The relevant New Hampshire and Massachusetts county court reports and possibly various state reports are to constitute a separate microfilm publication to be issued by University Microfilms in association with Dartmouth College. In addition, a supplementary reel will take care of miscellaneous items that keep on "dribbling in" to the editors at Dartmouth. Thus the microfilm reproductions will eventually include practically all the Webster items that can be located—with the exception of a few manuscripts in the hands of private collectors who have refused to let anyone copy or even look at their treasures.

The printed *Guide* includes a brief account of the present enterprise and previous collections and publications of Webster's writings; a

Webster chronology; a selective bibliography of Websteriana; a summary of the contents of the microfilm, reel by reel; and, most important, an index to the microfilm. The index appears to be fully adequate for the correspondence, listing every letter by the name of the sender or recipient (when known), but is of little use for the rest of the material, providing only a few, very general entries, such as "Congresses, documents relating to: 13th Congress . . . 14th Congress. . . ." The bibliography, in a curious oversight, omits the standard biography by Claude M. Fuess (1930).

For a definitive edition of a statesman's works, microfilm has obvious advantages over letterpress. In this case, the same staff would probably have required four times as long to prepare the material for printing, and the total price of the volumes would have been about double that of the reels. While of little interest to the general reader, the books would be of less value than the microfilm for the specializing historian, who prefers reproductions of the actual manuscripts, despite their many obscurities. The editors of the Webster papers, keeping the historian rather than the archivist in mind, have taken pains to lessen the difficulties while retaining the advantages of the original documents. These were first copied by xerox, and the copies were arranged in chronological order, pieces being cut and rearranged where necessary, before being microfilmed. The result is a continuous and consecutive series, saving the reader the necessity of searching through dozens and even hundreds of separately filmed collections, winding and rewinding reels, and occasionally "turning his viewer or his head to read vertical or inverted lines." Each item is carefully identified and its source is indicated.

But the editors are not neglecting the general reader or the student who is more interested in the period than in the man. They are also preparing a selective letterpress edition, which will run to several volumes and will include cross references serving as an additional guide to the microfilm and indicating what has been left out of the printed set. The first volume is expected to appear in the fall of 1972.

All in all, this entire project deserves the highest praise. Not only will it provide rich resources for American history during the long

period of Webster's prominence and rescue the man himself from the comparative neglect into which he has fallen; it will also serve as a model for other editorial projects in the future.

RICHARD N. CURRENT
*University of North Carolina,
 Greensboro*

DEE BROWN. *Bury My Heart at Wounded Knee: An Indian History of the American West*. New York: Holt, Rinehart and Winston. 1970. Pp. xvii, 487. \$10.95.

Bury My Heart at Wounded Knee is a sustained and powerful indictment of the treatment of the American Indians by the United States government. In dealing with Indian-white relations in the period from 1860 to 1890, Dee Brown has returned to well-known episodes in the tragic story of injustice and misery—the roundup of the Navajos, the Sioux uprising in Minnesota, the Sand Creek massacre, the war for the Powder River, the Modoc war, the Red River war, Custer's defeat, the flight of the Nez Percés, and Wounded Knee, to say nothing of the troubles of the Cheyennes, the Poncas, the Utes, and the Apaches. Among the actors, too, whose portraits grace this attractively printed volume, there are many familiar persons: Red Cloud, Spotted Tail, Cochise, Captain Jack, Sitting Bull, Crazy Horse, Chief Joseph, Ouray, and Geronimo, to mention only a few. For most students of the American West the events narrated will be familiar, for despite the author's claim that he is telling a new story out of "sources of almost forgotten oral history," the book uses standard sources that have been well used by others, and in fact there is much evidence in the book that it rests heavily on these previous works. What separates the book from many previous historical treatments of Indian wars and Indian policy is its one-dimensional concern with the plunder of the Indian lands, the guilt of the white man, and the glory of the Indians through it all.

This is the kind of book that would ordinarily not be noticed by scholarly journals in their review pages, but the wide sales of the book, the praise given it by uncritical and unknowledgeable reviewers, and the advertising of the publishers, who hope to have the book used in college courses, prompts a careful appraisal of

the book and of the author's techniques. The book is put forward as "the first book which thoroughly documents the history" from the Indian side, and the naive reviewer in a popular news magazine said that Brown's "scrupulous documentation of the events and words that he records makes me trust in his fairness." That the book is thoroughly or scrupulously documented is simply not true. No attempt is made to indicate the full sources from which the story is constructed. For the chapter on the Navajos, indeed, the author culled material without any sort of acknowledgement from an unpublished manuscript he was reviewing for a university press. Footnotes are limited to the direct quotations used in the book, and often the quotations are taken from secondary sources. Since many of the footnotes, however, refer to government reports, the unwary reader may falsely assume that the book is a scholarly historical work.

The materials have been selected to make the author's point, not to present a balanced view of what happened, from the Indian's standpoint or from any other. Many of the quotations do not follow the original sources exactly; some of the direct discourse is in fact fabricated, no doubt to give more dramatic effect and immediacy to the story. In the chapter on Ely S. Parker, the Seneca Indian who was President Grant's commissioner of Indian affairs, the use of material is subtly dishonest.

The book contains a great many errors of fact; some of them are minor, but they add up to considerable misinformation given to the reader. And the footnotes have been carelessly proofread. What is more important, however, is the error that comes from the author's selectivity. He does not set his story in a wide enough context to give it needed perspective. He scarcely hints at the serious concern for Indian rights on the part of the Indian Office and humanitarian reformers and the moves for Indian welfare that they effected. It is hardly acceptable, for example, to discuss the Ponca affair of the 1870s without so much as a mention of the humanitarian reaction or of the government's indemnity to the Poncas for what it admitted to have been a serious blunder.

It is to be regretted that this sort of "Indian history of the American west" gains such popu-

lar acceptance. The Indians, seriously wronged as they often have been, certainly deserve something better.

FRANCIS PAUL PRUCHA
Marquette University

ROBERT G. ATHEARN. *Union Pacific Country*. Chicago: Rand McNally and Company. 1971. Pp. 480. \$15.00.

Historians of the American West have long awaited the opening of the archives of the Union Pacific Railway, and interest has mounted since the announcement six years ago that Robert G. Athearn was to be given access. As usual when company records become available to historians, few skeletons were found secreted in the files, not even anything new on scandals as well as known as the *Crédit Mobilier* of America. In fact, Athearn reports that there is surprisingly little on the building of the original line. The correspondence of the company's presidents, Oliver Ames, Sidney Dillon, and Charles Francis Adams, provided the bulk of new information.

Athearn undoubtedly welcomed this turn of events for he had little interest in writing still another account of "the race to Promontory." Instead he concentrated on the later construction of feeder lines reaching out into lands more productive than those traversed by the main trunk. Thus, he has explained how the Western country was penetrated, settled, and developed with the aid of the Union Pacific, rather than writing a company history. This approach to railroad history provided the author with a broad canvas and a wide brush so that he could use his well-known talents to the fullest.

Utilizing a chronological-geographic approach, the story unfolds with the establishment of Union Pacific control over the first branches, the Kansas Pacific, Denver Pacific, and Utah Central. Subsequent construction efforts are organized around four frames of reference: the penetration of the Colorado Rockies, spur lines to serve the residents of the Plains of Kansas and Nebraska, the intermountain roads in Utah and to the mining camps of Idaho and Montana, and finally the move into the Pacific Northwest.

If not outright sympathetic to the railroad

executives, the tone of the book emphasizes the positive side of their efforts. The reader is informed that financial profit was incidental in disposing of the railroad's extensive land grants, that locating settlers who would make the land productive was far more important. Moreover, the Union Pacific had so much competition from the government and other railroads with land to dispose of that prices received in many areas were ridiculously low. The railroad, rather than prospective farmers, appears to have been the victim of the government's land policies and administration that delayed the issuance of patents and the surveying necessary for sale. The railroad did enjoy exemption from taxation in the interim but was rewarded with hostility from states that lost revenue. In reading of events leading to receivership and bankruptcy in the 1890s, one gets the impression that the government was again a millstone around the neck of the Union Pacific, bickering about the repayment of loans and restricting the company so it could not fully demonstrate its entrepreneurial ability in competition with other lines that had no financial obligation. This viewpoint parallels that of other historians who have recently suggested that all the initial financial manipulations of the builders were necessary because of the unrealistic requirements of the government to protect loans. The continuous understanding, even concern, for the railroad leaders that emerges from the book inevitably results from Athearn's decision to have them tell of their problems by extensive and effective statements from their hitherto unavailable letters. The closing date of the book, 1893, coincides with the shut-off date of material in the archives at Omaha.

The bibliography testifies to the extensive research for this study. Second in value only to the company files, in Portland as well as Omaha, were the records in the Church of Jesus Christ of Latter-Day Saints Library in Salt Lake City. As a result the discussion of railroad building in Utah and the interaction between officials of the church and the railroad are far more penetrating than those on other regions where the author had to rely heavily on newspapers and occasional memoirs for parallel evidence to bolster or refute that of the railroad magnates. The text is profusely illustrated with

exceptionally relevant photographs published for the first time. Maps are so effectively distributed in the text that they alone reveal the pattern of the evolving railway network in Union Pacific country. Athearn clearly demonstrates his ability to conform to rigid standards of research demanded by the profession and at the same time to write a book that will be widely read and enjoyed. A short and thoughtful epilogue testifies to his maturity as a historian. In balance this volume is probably the best of the dozen books from his pen.

W. TURRENTINE JACKSON
*University of California,
 Davis*

C. DAVID TOMPKINS. *Senator Arthur H. Vandenberg: The Evolution of a Modern Republican, 1884-1945*. [East Lansing:] Michigan State University Press. 1970. Pp. viii, 312. \$9.00.

Arthur Vandenberg came into the 1920s a self-made, successful, rich man. He believed in diligence, frugality, and sobriety and in free enterprise, balanced budgets, the gold standard, and a government of checks and balances. He also believed that this country should stay away from entangling alliances and that "the axe of economy [must be] sunk to the roots of governmental extravagance and waste." He further believed in "*Babbitt* at his best" and that with "Harding at the helm we can sleep nights."

A good many people who felt the same way in the early twenties had a very hard time in the thirties and forties. Unchanging in their faiths and prejudices, they became marooned on islands of sterile opposition while the great currents of extraordinary change swirled round them.

Mr. Tompkins wishes to show how Vandenberg, starting from his initial premises, became, as a senator from Michigan in the years from 1928 to 1945, an increasingly useful public servant and, in the end, a statesman in this dreadful time.

The source of his strength was that while he kept in touch with his original presumptions he was prepared to advance beyond them—that is, to learn—and thus he was continually making selective accommodations between what he believed and what was actually happening. For three instances out of many: in

the early New Deal he opposed the NRA but took a very active part in the shaping of the New Deal's Federal Deposit Insurance Corporation; he was a faithful Republican, but he worked very hard to develop new political coalitions that could mount a more enlightened kind of opposition than he could discover within the party ranks; in the thirties he was a dedicated isolationist, but in the forties he took a leading part in the effort to make this country an active, responsible member of the family of nations.

In a time of great change it is sometimes hard to tell whether a man who makes such accommodations is a disciple of Edmund Burke, as the senator said he was, or a weather vane, as others claimed him to be. It is to the credit of both the senator and Mr. Tompkins that from these carefully prepared, well-documented pages Arthur Vandenberg emerges as a man of both perception and principle. Of course he was shrewd, and of course he was ambitious, but he also wanted to do, within the circumstances, the useful thing. This book is a valuable investigation of the nature of resourceful conservatism.

In writing Mr. Tompkins says that he followed Oscar Handlin's precept that for the biographer the subject is not the complete man or the complete society but the points at which the two interact. In his effort to keep to these points Mr. Tompkins has cut away a little too much of the man. Certainly Vandenberg was not Harry of the West or the Plumed Knight, but he was not as dull a dog as these pages suggest. One hopes that in his next volume, to cover the years from 1945 until the senator's death, that Mr. Tompkins will bring more of him to the surface.

ELTING E. MORISON
Yale University

JOHN O. KING. *Joseph Stephen Cullinan: A Study of Leadership in the Texas Petroleum Industry, 1897-1937*. Nashville: Vanderbilt University Press for the Texas Gulf Coast Historical Association. 1970. Pp. ix, 229. \$10.95.

The story of the petroleum industry in the United States and its leaders continues to be one of the most thoroughly researched and documented corners of industrial and business history. After the early fascination with the glam-

orous and swashbuckling dramas of alternating elation and disappointment during the discovery period, historians have settled down to writing valid analyses of how the United States found its principal fuel for the twentieth century. Professor King's clear and careful statement fleshes out the outlines of this story with still more detail.

The terminal dates listed by the subtitle are covered, but the main part of the story takes place in a scant decade and half after 1897. Almost forty per cent of the text is concerned with Cullinan's activities in the Corsicana, Texas, field, which just nudges into the twentieth century. In that era Texas emerged from being a state whose petroleum production placed it in competition with great areas like Spitzbergen and Graustark. But by 1901 Texas was fourth in the nation. With what subsequently took place in the discoveries at Spindletop, Sour Lake, and other internationally unheard-of places, Texas not only shot into the forefront of American crude production; but because of Standard Oil's excruciating experience with Texas antitrust laws, a wary Standard Oil refused to take advantage of this latest opportunity to buy into new flush production. The result was that whereas Standard Oil had controlled eighty-five per cent of the crude production in 1899, it accounted for only ten per cent of the crude production during most of the decade prior to its dissolution by the Supreme Court in 1911.

Through all of this progress Cullinan emerged as a Pennsylvanian turned Texan who could see beyond the exhilaration of production to its problems of collection, distribution, and conservation. The first abandonment legislation in Texas was practically written by him. The seed for the petroleum conservation laws that Texas has observed was also planted by Cullinan. While undoubtedly these laws have been used by the Texas regulatory agencies to maintain prices, undeniably they have also provided guidelines for other states in working out their own conservation problems.

King shows clearly how Cullinan helped form one of today's major companies, the Texas Company, which simply outgrew its hero. More and more often committees hacked out decisions until Cullinan was looked upon

as something of an antique. The founding father had no choice but to go.

The only errors in the book are too trivial to detail. The study contributes to our knowledge of the formation of a giant American enterprise and gives insight to the groping for maturity by that industry. The book will take its place alongside the other fine and solid studies in the American petroleum industry.

JOE B. FRANTZ

*University of Texas,
Austin*

STANLEY P. CAINE. *The Myth of a Progressive Reform: Railroad Regulation in Wisconsin, 1903-1910*. Madison: State Historical Society of Wisconsin. 1970. Pp. xvi, 226. \$7.95.

The reputation and historic importance of Progressive politics in the United States have long been linked to the move to regulate railroads and utilities by public agencies, and in this familiar story nothing has been more celebrated than the achievements of the state of Wisconsin under the prodding of Robert M. La Follette. Until the recent appearance of David Thelen's penetrating studies, and now Stanley Caine's book, most writers have tended to accept at face value the governor's own account of himself and his record. Caine has chosen to burrow deeply into one facet of the record—the first major effort to force a reduction in freight and passenger rates—in order to evaluate with some precision the aims, achievements, and limitations of American Progressivism.

Caine asserts two important and unfamiliar points. The first is that La Follette displayed a marked political ineptitude in failing to obtain the kind of regulation that he had found so profitable to promise in his campaign for governor. The second, growing out of the first, is that the Wisconsin Railroad Commission began and remained not as the activist, countervailing, adversary body envisioned in reform theory but as a conciliatory agency seeking to harmonize the interests of the railroads and the largest shippers in the state and so became one of the nation's more conservative regulatory bodies. La Follette appears to have overestimated greatly the public's concern for rate regulation and badly misjudged his control of the

legislature. Against determined railroad opposition he won the right to regulate but lost the battle to shape and manage the regulatory process. The Act of 1905 restricted the commission to piecemeal revision of individual rates; by contrast, most other states were permitting their commissions to reconstruct entire systems of rates. Wisconsin's first commissioners additionally construed their powers so narrowly as virtually to abandon their explicit right to initiate their own review of rates. Henceforth they would act only on specific complaints. By 1910, when an impatient legislature tried to limit passenger fares by statute to two cents a mile, the commission resisted the move, and railroad men came to count on it as an ally against legislatures, politicians, and reformers.

Caine's study reveals distinctive care and thoroughness in the use of sources (most notably the La Follette papers and the extensive records of the commission) and sensitivity to the role of individuals in the shaping of institutions. His bibliographical note is a model of effectiveness and good judgment. But though his findings are persuasive, some of his own conclusions are not. It does not follow that the commission failed to achieve fair rates simply because few rates fell between 1904 and 1911 while railroad profits rose, or that its valuation of railroad properties was excessive simply because its figures came close to the railroads' figures, or that preferential rates were undesirable because reformers complained about them. To argue these conclusions with care requires the kind of economic analysis that Caine does not provide. Progressive regulation of railroads and other utilities also embodied tax rates, inspection of books and operations, and limitations on financing, and Caine does not discuss these matters. What this book does best is to show, as its title implies, that although the reform politicians failed to achieve one of the things they set out to do, they later proclaimed that they had succeeded in doing it, and historians have generally believed them.

OTIS A. PEASE

University of Washington

DANIEL S. HIRSHFIELD. *The Lost Reform: The Campaign for Compulsory Health Insurance in the United States from 1932 to 1943.* (A

Commonwealth Fund Book.) Cambridge, Mass.: Harvard University Press. 1970. Pp. xi, 221. \$8.50.

The current debate over a national system of health insurance is more than a half-century old. As early as 1917 a model bill for compulsory medical insurance had been introduced in fifteen state legislatures, and even the American Medical Association had endorsed a form of compulsory insurance.

But the opposition of organized labor, the protests of commercial insurance companies, the subsequent switch in medical attitudes, and finally the nation's involvement in World War I routed the advocates of compulsory health insurance. During the 1920s the health reformers were largely silent or ineffective as New Era politicians squabbled over taxes, tariffs, and dams.

This slight book records the revival of reform efforts and their new defeats in the years from 1932 to 1943. The first battle was fought over efforts to include health care in the social security legislation that was shaped in the early New Deal. The reformers were beaten by a combination of public apathy, presidential caution, and the successful tactics of organized medicine. The AMA simply outmaneuvered and outfought the reformers in the battle for the mind of the president. At the same time the doctors supported voluntary hospitalization plans for workers and individuals that took part of the sting out of the rising costs of medical care.

By 1937, following the creation of programs authorized by the Social Security Act, the reformers were ready for a new try at health legislation. Again, the legislative proposals growing out of the National Health Conference of 1938 were defeated by organized medicine and the president's concern about public readiness to accept the principle of federal health insurance. After 1938 public and governmental attention turned increasingly to the growing crisis in Europe.

The author argues that a deep-seated change in attitudes and tactics about health insurance surfaced in the early 1940s, but he does not prove his thesis. His interpretation that the shift from a federal-state cooperative approach to a national health program in the Murray-

Wagner-Dingell Bill of 1943 was crucial in the ultimate success of Medicare and later governmental programs does not seem persuasive without more evidence.

The book is a limited study of a forgotten period in the health insurance crusade. It tends to draw heavily on materials and interviews from the reform group but explores too little the reasons for organized medicine's successful opposition to change. As the battle goes on in the present Congress, it may prove of some use in giving perspective and background to this "lost reform" of the Progressive and New Deal years.

THOMAS N. BONNER
University of New Hampshire

BRUCE M. STAVE. *The New Deal and the Last Hurrah: Pittsburgh Machine Politics*. [Pittsburgh:] University of Pittsburgh Press. 1970. Pp. x, 262. \$8.95.

Professor Stave ends his very excellent book with these words: "The New Deal brought to Pittsburgh and its Democratic committeemen more than Roosevelt's program for recovery and reform. On the municipal level it brought the beginning of an unbroken Democratic voting tradition that extended at least through the 1960's; it also furnished the patronage that served as the initial lubricant for a continually well-oiled machine. For Pittsburgh's Republicans the advent of the New Deal signified 'the Last Hurrah'; for the city's Democrats it sounded the First Hallelujah." This change came about despite the almost complete domination of Pennsylvania politics by the GOP for the two generations after the Civil War.

In his approach Professor Stave, who is a former student of Samuel P. Hayes at the University of Pittsburgh, supports his mentor in contending that "the machine was simply the political expression of inner city life" and "reform was a movement of the periphery against the center." Naturally this position puts Stave in apparent opposition to the theories of men like Richard Wade and Zane Miller. Professor Miller in his recent book *Boss Cox's Cincinnati* (1968) goes to great lengths to explain how in Cincinnati the three rings—the Circle, the Zone, and the Hilltops—cooperated to support the machine as long as it gave each of them what they desired. Again, the peculiar experi-

ence of each city might have been sufficiently different to lead these two men to different conclusions.

Stave also thinks that his research in Pittsburgh supports the role of Edwin O'Connor's fictional hero Frank Skeffington as well as Oscar Handlin's research in the Boston area regarding the importance played by the Irish-Americans in machine politics. Finally, by giving the specific details of what happened in a specific city—Pittsburgh—the author gives new insights into F.D.R.'s relationship with the "urban bosses," which enlarges upon what has already been presented by Edward J. Flynn, James Farley, Arthur Schlesinger, and Samuel Lubell.

This book is a detailed study of facts and statistics, and it is certainly one more example of the current trend toward taking an interdisciplinary approach to basic historical research. The book also demonstrates the weaknesses that such an approach can lead to. For instance, there are the obvious limitations of heavy reliance on local records, both in regard to their completeness and in regard to their availability. When viewed in its totality, however, Bruce Stave has produced a work that is both soundly researched and useful to other scholars because it fills in one more gap of knowledge in the area of the political effects of the New Deal on local parties and urban machines.

PAUL L. SIMON
Xavier University

NOEL STOCK. *The Life of Ezra Pound*. New York: Pantheon Books. 1970. Pp. xvii, 472. \$10.00.

One of the most discerning of Ezra Pound's friends and critics, William Butler Yeats, noted regretfully the discontinuity in which the poet's extraordinary lyrical gift seemed stubbornly embedded. Whether Pound at last achieved in *The Pisan Cantos* the fusion of meaning and form his admirers yearned for begs the question: why did this man whose critical theories and poetic innovations rank among the major contributions to twentieth-century literature ultimately fail to develop an integrated self as he passed beyond the integrities of youthful genius?

The answer is attempted by an Englishman

who brings to the study of American eccentricity a dispassionate and, at the right moments, a compassionate intelligence. The unwieldy facts of Pound's life repeatedly suggest riddles: Pound the translator, emerging from the sobrieties of American middle-class culture and a fascination with the colloquialisms of native speech, into a remarkable feeling for medieval and Chinese literature; Pound the willful esthetic purist selflessly promoting the work of unknowns; Pound the ideologue, railing with justice against the quality of American culture and then certifying the wisdom of a Mussolini; Pound the economist finding support for his obsession with monetary reform in the writings of successive generations of Adamsses. (His absorption with neglected chapters of American history extended to Van Buren's *Autobiography*, first made available to him as part of the Annual Report of the American Historical Association.)

The oddities of Pound's nature are not so much psychologically probed as they are carefully related to the development of his extremist positions. In this respect Stock enhances our understanding of the origins of many of Pound's fabled pronouncements on art and politics. Stock's comprehensiveness can be excessive, occasionally obscuring the narrative thread. Also, commendably, it leaves us little opportunity to extract from his subject any simple determination as to which bears the greater onus for the pathos of Pound's confinement in later years—the inadequacies of self or the inadequacies of society.

ANNETTE K. BAXTER
Barnard College

W. J. ECCLES. *The Canadian Frontier, 1534-1760*. (Histories of the American Frontier.) New York: Holt, Rinehart and Winston, 1969. Pp. xv, 234. \$4.95.

The first question the author had to ask himself was what is meant by the frontier in New France. The answer that emerges is that it was not the frontier that was so long the staple of American historiography. It was not, as he illustrates, that receding area of free land and westward advance of settlement that appeared to account for so much of America's development. Rather the Canadian frontier "consisted of a main base on a river that gave easy

access to the heart of the continent, and several smaller bases . . . far in the interior but dependent on the main base, which in turn was dependent on the mother country, France." For much of its existence this vaguely defined dominion sustained a great variety of activities—missions, trade, diplomacy, war, settlement, and administration—each of which, it would appear, helped to fashion a distinctive dimension of the Canadian frontier. (Anyone familiar with Britain's expansion in the Southern Hemisphere in the nineteenth century might be tempted to compare that with what this book indicates the French sought to do in the Northern Hemisphere a century or two earlier.)

Eccles has contributed a great deal to a reappraisal of New France's past as he ranges from an examination of the community's inherited social values to the way in which it succumbed to the British in the Seven Years' War. There is much here that explodes hoary misconceptions and ancient distortions. In his analysis, for example, of New France's society and administration he offers the quite convincing argument that it was the metropolis overseas and not the frontier (or the "environment") that left the most indelible mark on the colony. Obviously the wilderness made some modifications in the European life style (the Indianization of the *coureurs de bois* is a case in point); but the political ideas, commercial ambitions, and administrative concepts imported from the metropolis probably counted for far more than the frontier in the shaping of the basic institutions of New France. It is refreshing to see this sort of treatment in print.

As regards Indian-white relations upon which trade and missions abjectly depended, the author backs the thesis (though the editor of the series of which this volume forms a part would have us believe that Eccles invented it) that the Indian was no mere pawn in a European commercial game but was a vigorous middleman in his own right, fully capable at times of playing off one power against another. Although, generally speaking, this vital section of the book is lucidly presented and appears to be soundly researched, it has in a number of places an odor of ethnocentricity about it. A scholar who has made his reputation in part, to quote the editor, by "trampling legends" and "toppling reli-

gious beliefs" is also capable of making the following statement: "Lacking the inhibitions that control the subconscious of Europeans, to some extent at least, once drunk [the Indians] ran amok." Bibliographically as well this section seems to have been mildly shortchanged for there is in the notes no mention made of the recent work done by Bruce Trigger on Indian-white contact in New France.

Finally, the book seems to lack a satisfying conclusion. There is, to be sure, an epilogue but no statement that would tie together the varied arguments and analyses that the author has painstakingly paraded before the reader in the body of the work.

C. M. JOHNSTON
McMaster University

ROBERT RUMILLY. *Histoire de Montréal*. In two volumes. Montreal: Fides. 1970. Pp. 474; 418.

Montreal is one of the great cities of North America. For Canada this brilliant, bilingual metropolis has been and will continue to be of immense significance. In the economic life of the country, in federal and Quebec politics, in religion, education, and culture, Montreal's importance is beyond dispute. Clearly, therefore, the city deserves, and is a fit subject for, a first-rate written history. But, though several historians (mostly amateurs) have tried, no one has yet produced a satisfactory work in either English or French.

Unfortunately, Robert Rumilly is no exception. These two volumes contain a general account of the city's development from its origin as Ville Marie to the mid-nineteenth century. The intention is ambitious. To review the result, however, is to write a catalog of inadequacies. The author dismisses or ignores much recent scholarly research bearing on his subject. He has made too much use of the readily available Roman Catholic ecclesiastical records with the predictable result that the narrative is somewhat distorted. His conceptual framework and the analyses he makes are insufficiently developed—indeed, in this respect the work could easily have been produced forty years ago. There is practically no attempt to relate the development of the city either to national history or to the history of Quebec. One could

go on; but let it be sufficient to say that the history of Montreal remains to be written.

H. E. TURNER
McMaster University

CLAUDE GALARNEAU. *La France devant l'opinion canadienne (1760-1815)*. Preface by ANDRÉ LATREILLE. (Cahiers de l'Institut d'Histoire, Number 16.) Quebec: Presses de l'Université Laval. 1970. Pp. xi, 401. \$10.00.

In a thoughtful and meticulously researched study, graced with a superb bibliography, the author succeeds in identifying the successive opinions and beliefs French Canadians held relative to France, their motherland, in the half-century following the British conquest. The greatest contribution offered by this volume is that it surpasses mere description of Canadian reactions to the anticlericalism and republican excesses of Old France, or of the Anglo-French confrontation and Franco-American alliance, in order to demonstrate the relationship between European events and the process of defining Canada's role and place in the historical stream. One would have appreciated, however, a more precise definition of "opinion" than that offered by the author since it is evident that the sources employed, largely newspapers with some private correspondence, reflected the views of an elite, not the Canadian masses, and are supported in some instances solely by quotations from His Majesty's "old subjects," not French Canadian "new subjects." Nevertheless, the author indicates how each generation interprets its own history and how succeeding generations may have imposed on them views of their past arising out of a particular juncture in international relationships.

Professor Galarneau, in offering a convincing account of the development of an ideology, has rendered imperative the reappraisal of much accepted history. Requiring review are Trudel's view of Voltaire's unique influence in Canada, Flenley's assertion that ecclesiastical censure suppressed intellectual activity, Vernon's thesis that the British administrators were unalterably opposed to the French Revolution from its outset, Wade's assumption that the French Revolution resulted in a rupture in cultural relations between France and Quebec,

and Brunet's accounts of enthusiastic support for a French reconquest.

By utilizing content analysis without tedious quantification Galarneau succeeds in showing that public opinion may be formed as much by what passes in silence as by what becomes the object of propaganda. Whatever the methodology employed, it is still the interpretive ability of the author that determines the relevance and significance of the research undertaken. *La France devant l'opinion canadienne* is a provocative and important extension of the work of Godechot and Palmer on the impact of the French Revolution.

CORNELIUS J. JAENEN
University of Ottawa

WILLIAM ORMSBY. *The Emergence of the Federal Concept in Canada, 1839-1845*. (Canadian Studies in History and Government, Number 14.) [Toronto:] University of Toronto Press. 1969. Pp. 151. \$4.75.

This little volume presents a great deal of material that is new or else presented in a new way. Ormsby argues not only that Lord Durham wanted a federal constitution for British North America but that he worked almost until the end of his stay in Canada trying to persuade the merchants in Montreal to accept such a system. He goes so far as to say that the substance of the Durham report did not take shape until Durham was on his way back to England. He also brings out what has not been stressed before—although some of the facts were stated by Chester New—that Durham used the plan drawn up by J. A. Roebuck as a model in trying to convert the Montreal merchants to accept a federal system. An article by R. S. Neale, which appeared recently in *Historical Studies* published in Melbourne, analyzes that plan and states that it was founded, first, on a desire to protect the interests of the French Canadians and, second, on the American plan for the relations between the executive and the legislature. That being so, it seems inevitable that Durham should have dropped it when he found the French Canadians uncooperative and offering the threat of another rebellion. He was on much safer ground in accepting the ideas of Robert Baldwin, whom he met in Toronto.

Ormsby is convinced that the British government made a mistake in not introducing federal government in 1840 even though it would, in the beginning, have been merely the federation of the two provinces. To me, such a plan seems to be no solution at all. The only hope for Canada was that the French Canadians should be compelled to cooperate with the reformers of the Upper Province; if the Lower Canada assembly had been allowed to meet again it would inevitably have continued to pass resolutions on the subject of its own grievances and would have paid scant attention to any federal parliament. That is what both Lord Durham and Lord John Russell came to realize, and while they hoped, no doubt, that in a united province the French would very soon be absorbed and assimilated by an increasing British population, Russell was persuaded eight years later to accept cabinet government, in which the French had an equal voice with the English-speaking colonists in making the most important decisions. Very few politicians in Canada thought of the system of government in the United Province as working smoothly or well, but in the 1850s it became the model, as far as the Colonial Office was concerned, for responsible government as it was being set up in Australia and New Zealand.

HELEN TAFT MANNING
Bryn Mawr College

ELIZABETH NISH, editor. *Debates of the Legislative Assembly of United Canada*. Volume 1, 1841. (Debates of the Legislative Assembly of United Canada, 1841-1867. Published under the direction of the Centre d'Étude du Québec and the Centre de recherche en histoire économique du Canada français.) Quebec: Presses de l'École des Hautes études commerciales. 1970. Pp. civ, 1099.

Historiography in Canada has suffered from the absence of published parliamentary debates for the first seventy-five years of the nineteenth century. Even in 1867 the Parliament of the new Dominion of Canada refused to print its debates. The reasons were many. Cost was one; and since most newspapers published abbreviated reports, the cost seemed unnecessary. The prolixity of parliamentarians was another. Even at the best of times M.P.'s found it diffi-

cult to keep their lucubrations to a decent minimum; when it was known that speeches would be fully reported for posterity, the result was often impossible as to both length and dullness. The debates on Confederation, in the Parliament of the Province of Canada in 1865, reported officially, confirmed the worst fears of opponents of official debates.

Recently there have been attempts to fill in gaps left by our sensible ancestors, to aid our own reconstruction of their life and times. The Parliament of Canada has already authorized the publication of its missing debates between 1867 and 1874. This book is the beginning of another series, to fill in the twenty-six years of missing debates in the Province of Canada from 1841 to 1866. (The Province of Canada was a legislative union of early forms of the present provinces of Ontario and Quebec.)

This first volume, for 1841, is a superb piece of work. The introduction in particular is masterly. It is a substantial history of parliamentary reporting in England and in the Province of Canada, written with verve and a good deal of research behind it. Much parliamentary life also comes through. During a press boycott of the Assembly in 1850, it was clear that M.P.'s missed having their speeches reported in the newspapers. An M.P. rising to speak "casts a look at the [reporters'] gallery, and recollecting that he is only addressing a dozen or two persons, one half of whom are probably asleep, and the other not listening, down he drops again fairly dumbfounded [*sic*]." This from the *Montreal Pilot*, July 27, 1850.

Perhaps the most awesome aspect of this reconstruction is the methodology of it, which explains both the expense of this volume and its excellence. The whole book is compiled from eight different newspapers, including one French-Canadian, using the *Journals* of the House as the essential framework within which to fit the result.

One may quarrel with minor aspects of format or some printing errors, but the whole work is a considerable achievement. The editor, Elizabeth Nish, is to be congratulated on an admirable not to say monumental piece of work. My only complaint is that there is not a single reference to any of the other colonies of British North America—Newfoundland, Nova Scotia, New Brunswick, Prince Edward Is-

land—all of whom had their own practice and their own debates. But since so many good things have already been given in that introduction (some ninety-five pages), it is perhaps unreasonable to ask for more.

P. B. WAITE

Dalhousie University

DONALD CREIGHTON. *Canada's First Century, 1867-1967*. New York: St. Martin's Press. 1970. Pp. 372. \$10.00.

This work is an interpretative narrative of Canadian history from the formation of the federal union to the present. For more factual accounts of the same period one must consult textbooks written for college students and the as yet incomplete series of monographs comprising the Centennial History of Canada.

To Canadians, the publication of a new book by this author is always an event in the intellectual history of the nation. For over forty years he has taught in Canada's largest and most influential department of history—at the University of Toronto—where he has been the most sought-after supervisor of graduate students specializing in Canadian history. During this period Creighton has published six books whose central concept can only be compared in its influence to Turner's frontier thesis. His "Laurentian theme" postulates that the St. Lawrence River has inspired successive generations of far-sighted and influential Canadians to build a great territorial empire in North America. To achieve this objective and to maintain its integrity, the Fathers of Confederation sought to create a federation with a strong central government that would be the well-armed champion of national unity in any contest with provincialism. Externally, the independence of the nation was to be based on an Anglo-Canadian political, military, and economic alliance that would evolve naturally out of the older imperial-colonial relationship.

Thus Creighton's nationalism was based on what he regarded as the most creative, constructive forces in the last two centuries of Canadian history; not isolationist, but outward-looking to the British Commonwealth and to the two great twentieth-century structures of international cooperation, the League of Nations and the United Nations; and confident of

the prospects for indigenous cultural achievement.

As recently as a decade ago Creighton was sanguine in his assessments of the vitality of these ideas and influences in Canadian life, but this latest work is profoundly pessimistic and reinterprets his previous treatment of recent history in terms of the decline of Canada's national independence and identity. In developing this new interpretation he eliminates or contradicts many of the assessments of twentieth-century Canadian affairs that appear in the 1957 edition of his *Dominion of the North*. There is a new asperity in his attitude to the influence and objectives of French Canadians; he deplores the popularization of the view that confederation was a cultural compact between two ethnic communities, rather than a political union. He is as concerned as the articulate young leftists with the influence of American capitalist enterprise, which has made Canada an economic satellite of the United States and is equally appalled by the heavy-handed presidential and state department intervention in Canadian affairs in furthering the psychopathic American crusade against international communism. The colonial mentality of the Conservatives as a brake on the development of Canadian national feeling is largely ignored, and his severest criticism is reserved for the leadership of the Liberal party since 1919 as the chief instrument of a continentalist reorientation of Canadian affairs, although Creighton does not exempt the average Canadian's enjoyment of the fleshpots of his affluent neighbor. There are, however, an increasing number of Canadians who reject his idea that they have been "left without the power to fashion a new future for themselves," but only time will prove whether theirs is a vain aspiration.

LEWIS H. THOMAS
University of Alberta

CARL BERGER. *The Sense of Power: Studies in the Ideas of Canadian Imperialism, 1867-1914*. [Toronto:] University of Toronto Press. 1970. Pp. 277. \$10.95.

DESMOND MORTON. *Ministers and Generals: Politics and the Canadian Militia, 1868-1904*. [Toronto:] University of Toronto Press. 1970. Pp. x, 257. \$8.50.

Because Canada's first half century was a pe-

riod of adolescence when aspirations for autonomy were hampered by weakness and dependence, Canadian history usually has been written as a struggle to accommodate or overcome political, geographic, and economic circumstances in which the dominant figures of the age, Macdonald and Laurier, despite different backgrounds and party affiliation, contributed to Canadian national development while simultaneously maintaining the British connection.

The Sense of Power is the first look at one aspect of relevant ideology affecting the achievement of an independent Canada within the Commonwealth. Berger exhaustively mined the extant writing and utterances of a group of Canadian imperialists, the militia colonel George Taylor Denison, III, the principal of Queen's College George Munro Grant, the headmaster of Upper Canada college (later Rhodes Trust secretary) Sir George Robert Parkin, the economics professor and humorist Stephen Leacock, and the literary periodical editor Sir Andrew Macphail. He concludes that for these five, imperialism meant not British domination and exploitation but an opportunity for Canada to participate and perhaps even play a leading role in expansion. He postulates that there was a considerable degree of integration between Canadian imperialism and Canadian nationalism. Previously one had had an impression that there was dichotomy. This book is a sensitive and stimulating analysis of the history of an important segment of Canadian thought.

Berger leaves unanswered the question whether the prolific utterances of this handful of publicists represent the views of a considerable but inarticulate Canadian opinion. Pro-British sentiment was certainly widespread in many areas and was mingled with an assumption of a right to criticize freely but, as the sterility of imperial federalism amply demonstrated, it needed more than imperial sentiments and nationalist attitudes to produce a tight, viable imperial structure that could fully recognize Canadian ambitions.

Ministers and Generals gives a partial explanation of this dilemma of imperialism in Canada. Like *The Sense of Power*, it is based on the study of a limited number of the leaders of the period of adolescence, in this case the eight

British generals who commanded the Canadian militia. Morton has produced the first historical study of the militia based on documentary sources rather than merely on published reports and criticisms. His book is a penetrating study of the post-Confederation forces affecting the evolution of military institutions that were an important, if not an essential, element in the growth of Canadian nationhood.

Morton shows that during Canada's adolescence the British G.O.C.'s were thwarted by Canadian political pressures, which they despised. These were more than the usual civil-military frictions of a democracy. The soldiers in fact served two political masters, Britain and Canada, with different interests. Some of them were second-rate men of inferior caliber. All appeared to be unsuccessful. Yet collectively they introduced a military organization modeled on the British Army and fostered a strong military tradition and some expertise in an important minority of the Canadian population. Morton might perhaps have emphasized more strongly that, although all but one of the first of the eight failed to complete their proposed term of office, these British officers made a valuable contribution toward the military prestige Canada was to gain later in the First World War. For that prestige did not all stem from reforms instituted after the appointment of the G.O.C.'s was abolished in 1904 or from wartime improvisation. It had roots deep in the traditions and practices established during the nineteenth century. Nevertheless, in the long run it was Canadian national self-confidence rather than closer imperial ties that emerged from the experiences of war and peacemaking. The forces of decentralization to satisfy divergent interests were too strong for an effective imperial partnership to develop.

RICHARD A. PRESTON
Duke University

PIERRE BERTON. *The National Dream: The Great Railway, 1871-1881*. Toronto: McClelland and Stewart. 1970. Pp. xiii, 439. \$10.00.

This history covers the period from the time when the subject of a railway to the Pacific was first broached in the House of Commons to the passing of the Canadian Pacific Railway Act on February 15, 1881. It therefore deals with the

significant, yet frequently overlooked, period of government railway surveys and construction. The characters and personalities, duplicity, heroism, strengths, and weaknesses of the leading figures—politicians, financiers, contractors, and promoters—come through clearly. The ever present fact of politics stands out. The story of the "Pacific scandal" is full and circumstantial. A section aptly entitled "Sodom-on-the-Lake" provides a fresh and colorful picture of the seamy side of the life of railroad construction crews. The best parts of the book are those that tell of the practical aspects of railroad building—the difficulties and dangers faced by surveyors and construction workers.

Instead of footnotes, the author has supplied references, with no indication in the narrative, to his major statements and quotations identified by page and line, which must be used somewhat awkwardly in conjunction with the bibliography. These references average about three to the page. A limited check revealed some incorrect pagination and inexact quotation.

The bibliography is comprehensive. The author does, however, make categorical statements sometimes based on irrefutable primary sources and other times on doubtful secondary sources. A few significant statements are not supported in any way. Harold Innis's work on the C.P.R., recently reprinted, is not listed in the bibliography: his first hundred pages might have proved suggestive. Nine maps, drawn by Courtney C. J. Bond, are clear and useful.

This work with its wealth of colorful detail, often not directly related to the subject in hand, will prove most informative to the general reader for whom it is apparently designed and interesting to the serious student of Canadian history, who, however, stands to gain little new in fact or interpretation from its perusal.

On a recent trip from Toronto to Vancouver and back by C.P.R., with the review in mind, I questioned several fellow travelers and found them interested and well informed on the history of the railway, almost certainly reflecting the impact on laymen that this work has already made, and will continue to make.

A second volume is planned.

JAMES J. TALMAN
University of Western Ontario

MARTIN ROBIN. *Radical Politics and Canadian Labour, 1880-1930*. Kingston, Ont.: Industrial Relations Centre, Queen's University. 1968. Pp. xi, 321. \$7.95.

Professor Robin's study is a synthesis drawn from scattered materials relating to labor unions and political movements that were active in Ontario and Western Canada. These materials include government publications and the proceedings of union conventions. While labor newspapers were used, they are not included in the select bibliography. While this book is a contribution to Canadian political history, the extensive use of details in places tends to confuse rather than to enlighten the reader. Certainly, there were problems of selection since the book deals not with labor unions or radical political movements, as such, but with the relations between them.

According to Professor Robin, until the political discipline exercised by the regular political parties among organized workers had been eroded, a viable socialist party—the Cooperative Commonwealth Federation—could not be established. In order to throw light on this process, he examines the interplay of socialist movements, syndicalist movements, independent labor parties, and labor unions. Through a discussion of the attempts to form farmer-labor alliances during the period of agrarian political upheaval, the author supplements existing biographies of radical political leaders, the Progressive movement and the Winnipeg General Strike of 1919. Taking issue with economic determinism, the author concludes that there was no direct relationship between bursts of labor political activity and periods of business depression and that political radicalism was espoused more often by skilled than by unskilled workers.

Professor Robin, pointing out British Labour party influences, notes that by 1930 the predominant ideologies among organized workers were those of British democratic socialism and "labourism." The "voluntarism" of Samuel Gompers could be rejected even by "international" unionists because there was no "job conscious" antisocialist ideology among Canadian union leaders. Indeed, the author argues that while the "international" (American) unions did restrict independent labor political ac-

tivity at times, the "national" (Canadian) unions were conservative and not radical in their political ideology. Therefore, this work has some relevance for the perennial debate concerning the nature of Canadian nationalism.

G. E. PANTING

*Memorial University of
Newfoundland*

JOSEPH LEVITT. *Henri Bourassa and the Golden Calf: The Social Program of the Nationalists of Quebec (1900-1914)*. (Cahiers d'histoire, Number 3.) Ottawa: Éditions de l'Université d'Ottawa. 1969. Pp. ix, 178.

Although the main emphasis of this book is on the economic and social ideas of Henri Bourassa, the author also devotes considerable attention to other prominent Quebec nationalists of the time, such as Olivar Asselin, Omer Héroux, and Armand Lavergne. The various chapters discuss the attitude of the nationalists toward labor and social problems, colonization, the educational system, and the role of the monopolies and trusts in the corruption of Quebec politicians.

The main concern of Bourassa and his colleagues was the impact of the new foreign-dominated industrial system on traditional Quebec society. Their dilemma was whether to urge French Canadians to participate fully in this new economic system or to turn their backs on it and try to maintain a society with a strong rural base, with extensive colonization projects to stem the flight from the land. In the end they decided that industrial capitalism was here to stay and that the thing to do was to reform it. One proposal often put forward was for the province to nationalize, or at least more fully regulate, English-owned monopolies, such as telephones, gas, electricity, and railways. However, the nationalists, and particularly Bourassa, were limited in their proposals for reform by the principles of social Catholicism with its distaste for excessive state intervention.

One of the shortcomings of this book is that the various chapters seem like a collection of disconnected essays with not enough emphasis on a central theme. Another criticism would be that in many places the author is too dependent on secondary sources, which he uses too uncritically. Originally a doctoral dissertation, the

book would have been strengthened by more revision and reorganization. However, it does make the point that the nationalists were not quite as conservative in their economic thinking as is sometimes thought. The book contains a helpful bibliography.

HERBERT F. QUINN

Sir George Williams University

L. B. PEARSON. *Words and Occasions: An Anthology of Speeches and Articles Selected from His Papers*. [Cambridge, Mass.:] Harvard University Press. 1970. Pp. xiv, 296. \$10.00.

There once was a young Canadian minister's son who, judging from the extracts he himself has selected from his own writings and speeches, might have made a good sports columnist. He went wrong somewhere and wound up his career as prime minister of Canada.

This book provides some clues to explain how it happened. The selection of Lester Pearson's youthful essays shows a sense of wit and style as well as a wholesome, one might almost call it "distinctively Canadian," attitude on the subject of amateurism and professionalism in athletics. As a budding young history lecturer at the University of Toronto, Pearson also wrote a guide to students on "psyching-out" readers of term papers that has many points that are still valid. The "de-Bunker Hill" school of interpretation is again very much in fashion.

A change developed from the time he joined the Department of External Affairs in 1928. From that point on the bland clichés began to dominate his style—the formalized introductory jokes (some of which are good) followed by the usual routine summary of the world's crises from the economic problems of the 1930s to the cold-war rhetoric of the 1950s, with side trips to extol the blessings of international cooperation generally and the Commonwealth of Nations in particular. The reader's impression of these utterances is confirmed by a photograph (p. 62) of orator Pearson at the San Francisco Conference of 1945, surrounded by a group of auditors with uniformly glazed expressions on their faces.

Another less perceptible change occurred in 1948 when Pearson the civil servant became Pearson the politician, but this was the period of almost undisputed Liberal dominance, in-

cluding adjustment to and full rhetorical acceptance of the developing cold war, including NATO, Korea, and the rest. Pearson was a gracious loser when out of power (he was leader of the Opposition from 1958 to 1963) and a casual prime minister who lacked a parliamentary majority from 1963 until his retirement in 1968.

Historians judging the Pearson government's contributions to Canada can only cast interim conclusions aided by the memoirs of one cabinet colleague (the extremely harsh judgment of Judy LaMarsh's *Memoirs of a Bird in a Gilded Cage* [1969]) and the perceptive reporting of one journalist (the anecdotal narrative of Peter Newman's *A Nation Divided* [1968]). This book, containing short introductions and selected prose, inevitably emphasizes the more spectacular issues, such as the maple-leaf flag and the de Gaulle episode. I suspect that other issues, yet to be analyzed in full historical perspective, such as the diplomatic bargaining over rights and revenues between Quebec and the federal government and the establishment of pension and medicare plans, are monuments of greater significance to the Pearson legacy.

JOSEPH A. BOUDREAU

San Jose State College

J. W. PICKERSGILL and D. F. FORSTER. *The Mackenzie King Record*. Volume 3, 1945-1946; Volume 4, 1947-1948. [Toronto:] University of Toronto Press. 1970. Pp. viii, 424; 472. \$17.50 each, \$30.00 the set. Four-volume set \$50.00.

These volumes complete the publication of the record of Mackenzie King's career as portrayed in his diaries from 1939 to 1948. As before, the editors have allowed King to speak for himself through excerpts from his voluminous daily diary, providing only background information and connecting links where necessary. Volumes 3 and 4 make less absorbing reading than the first two covering the World War II years, but they are interesting and important sources for an understanding of Canadian history in the immediate postwar period.

The major interest in volume 3 lies in the Gouzenko spy case. Fearful that acute tension between the United States and Russia might lead to a movement for annexation to the United States, especially among western Canadians, and convinced that in the event of

atomic war Canada would be the most vulnerable nation on earth, King felt his responsibility in the Gouzenko affair, with its international ramifications, to be even greater than the burden he had borne on the eve of war in 1939. Characteristically exaggerating his own role in events, he could not interpret "how all this has come to my doorstep in any way other than . . . that I have been singled out as an instrument on the part of unseen forces to bring about the exposure that has now taken place."

Much of volume 4 concerns domestic politics after the war and especially the choice of King's successor. His account of the process by which the transition to the leadership of Louis St. Laurent was made without loss of unity or vigor in the party documents well the qualities that enabled Mackenzie King to lead the Liberal party for twenty-nine years, twenty-one of them as prime minister.

From the evidence of both these volumes it is clear that King's prewar policy of minimal Canadian commitments in international affairs did not change after the war. Although for reasons of his own and his country's prestige King was proud to have participated in the drafting of the United Nations Charter, he believed that the organization was nearly useless as an instrument of international peace since it lacked, inevitably and rightly, any power to enforce its decisions. Therefore, King expended considerable energy in curbing what he deemed the excessive enthusiasm of his advisers in the Department of External Affairs, notably Lester B. Pearson, for Canadian involvement in dangerous adventures, such as the U. N. Commissions on Korea and Palestine.

Historians with a psychological bent will find here, as in all portraits of Mackenzie King, ample scope for their skills in explaining the phenomenal political success of the lonely prime minister who could devote ten days to the illness of his dog, the superstitious bachelor whose acute self-doubt was allayed by the awareness of his mother's guidance from the unseen world, and the egalitarian so often profoundly moved by attention from the great.

King's literary executors have determined that the official biography begun by R. M. Dawson and continued by H. Blair Neatby will not go beyond 1939. That is regrettable; valuable as they are, the four volumes based on the

diaries for the later years do not take the place of biography. It is to be hoped that the decision will be reversed and also that in due course all the diaries will be open to historians generally.

MARGARET PRANG

University of British Columbia

RICHARD BOURNE. *Political Leaders of Latin America: Che Guevara, Alfredo Stroessner, Eduardo Frei Montalva, Juscelino Kubitschek, Carlos Lacerda, Eva Perón*. New York: Alfred A. Knopf. 1970. Pp. 310, x. \$8.95.

If *Political Leaders of Latin America* does not demonstrate anew that "politics makes strange bedfellows," it is by the author's choice at least an eclectic book. Starting from the thesis that politics in Latin America tends to polarize around personalities, Richard Bourne has assembled analytical accounts of six post-World War II leaders whose philosophies and tactics have been as diverse as the nations they led. His broad focus is the nature of political power in Latin America, the determinants of its exercise, and the restraints upon political behavior in general.

To illustrate the diversity he wishes to demonstrate, the author cites what he sees as the distinctive traits of his principals: the utopian vision, Robin Hood mystique, and guerrilla theoretics of Cuba's Che Guevara; the conservatism, ruthlessness, and indefatigability of Paraguay's Alfredo Stroessner; the probity and intellectual appeal of Chile's Eduardo Frei Montalva; the political coolness and pragmatic democracy of Brazil's Juscelino Kubitschek; the volatility, histrionics, and dialectical oppositionism of Brazil's Carlos Lacerda; and the demagoguery, iconoclasm, and vindictiveness of Argentina's Eva Perón. All of these except Stroessner and Kubitschek, though utilizing disparate techniques, claimed to be revolutionaries. Only two, Guevara and Frei, manifested an international outlook.

The author sandwiches the accounts of his "leaders" between an introduction, in which he depicts the socioeconomic milieu that produced them, and a conclusion, in which he introduces evolving factors that may change the character of future political personalities. Among these factors he emphasizes three: youth and its discovery of its own strength; the growth of cities

and the problems of megalopolis; and each nation's increasing awareness of other nations both within and without the hemisphere.

British-born correspondent for the *Manchester Guardian*, Bourne gathered material for his book on two extended visits to South America in 1966 and 1967. American readers will find provocative several allusions to United States posture toward Latin America. "One of the main fallacies of . . . the Alliance for Progress," he asserts, "was to imagine that a social revolution in the countryside could be achieved without revolutionary methods" (p. 36). In general, he suggests, United States attitudes are "a strange amalgam of idealism, contempt, frustration, and defensive self-interest" (p. 38).

Though one may wish his statistics—particularly on trade, military budgets, and population distribution—were more current, Bourne has produced a stimulating commentary, one probably more novel for the general reader and the undergraduate student than for the specialist.

HAROLD F. PETERSON
*State University College,
Buffalo*

HENRY A. LANDSBERGER, editor. *Latin American Peasant Movements*. Ithaca: Cornell University Press. 1969. Pp. xi, 476. \$12.50.

The situation of the rural population of Latin America has deep historical roots and has been a major preoccupation of twentieth-century reformist and revolutionary movements. The effort to organize, politicize, and mobilize the peasant population has been a logical consequence, and social scientists—including historians of that bent—have begun to undertake serious studies of peasant organizations and movements. This volume includes eight case studies of national, regional, or local peasant movements or organizations in seven Latin American countries.

In an introductory essay Landsberger, a sociologist, provides a theoretical framework combining the historical and sociological approaches, posing key questions deserving of consideration and empirical study by the contributors. The editor discusses a nine-point checklist identifying the information that is needed in order to understand the origins,

structure, and outcome of a peasant movement. Fifteen working hypotheses are offered as illustrative examples of how the individual researcher might analyze the long-run dynamics of the political and economic structure of the given society, the events, and the goals, mass base, allies and enemies, ideology, means, organization, successes, and failures of a peasant movement. The editor is thoughtful, balanced, and properly cautious. His framework is usable and adaptable.

Adapting specific research experience to this model in some instances proved difficult since individual contributors were utilizing data from previously conducted research. Comparability is also affected by the wide range of the dimensions of the geographical areas involved and of time span. The range extends from Landsberger's own study of a Chilean vineyard workers' strike in 1953 together with its historical antecedents and aftermath and Dwight Heath's anthropological report of peasant syndicates among the Aymara of the Yungas in Bolivia based on field work in 1964–65, through Cynthia Hewitt's account of the Pernambuco peasant movement in Brazil from 1961 through 1964, and Wesley W. Craig, Jr.'s examination of the peasant movement in La Convención Valley, ninety miles north of Cuzco in Peru, between 1952 and 1965, to Robert A. White's study of the Zapata movement and its relationship to the Mexican Revolution, John Duncan Powell's detailing of the thirty-year history of the Peasant Union movement in Venezuela, and an overview of Peruvian peasant organizations by Julio Cotler and Felipe Portocarrero underscoring regional variations between the coastal zone and the sierra. To me, one surprising fault is the infrequency of any reference to external influences, whether intrahemispheric or more broadly international.

Despite the limitations, the individual case studies are rich in material on the agrarian structure and the social, economic, and political contexts within which the peasant movement developed, on their leadership, ideology, and organization, and on their successes and failures.

Regarding evaluations of the movements, the concluding essay by Ernest Feder, an agricultural economist with long and meaningful ex-

perience in rural Latin America, examines the whole range of economic and political pressures constituting societal opposition to peasant movements. He stresses that the path to the attainment of a better life by Latin American peasants not only is strewn with individual hurdles representative of individuals and institutions committed to the status quo, but also is blocked by the opposition of a society hostile to the improvement of their lot.

The Landsberger volume represents a significant contribution to a field in which both research and resulting literature is in scant supply. Not only do the individual case studies provide rich material for the student of individual areas, but the framework proposed by the editor should prove invaluable for subsequent studies of Latin American peasant movements and organizations. Students and future investigators also should be well served by the geographically organized bibliography on Latin American Peasant Organization prepared by Gerrit Huizer and Cynthia N. Hewitt.

STANLEY R. ROSS
University of Texas,
Austin

JOHN V. LOMBARDI. *The Decline and Abolition of Negro Slavery in Venezuela, 1820-1854*. (Contribution in Afro-American and African Studies, Number 7. A Negro Universities Press Publication.) Westport, Conn.: Greenwood Publishing Corporation. 1971. Pp. xviii, 217. \$11.00.

In an attempt to broaden the scope of future comparative analyses of slavery in the Americas, Lombardi has undertaken to write "a detailed case study of the decline and abolition of Negro slavery in a country where slavery did not dominate the economy or society." In fact he has produced not a study of slavery but an overview of the politics and economics of abolition; the book deals more with white men than with black.

Except for a brief flourishing during the cacao boom of the mid-eighteenth century, slave importation was a minor activity in colonial Venezuela, and with the emergence of the independence movement in 1810 the slave trade was effectively terminated. During the wars of independence, the number of slaves decreased from approximately 60,000 to 40,000.

Yet, despite "an almost total unanimity of opinion against the servile institution"—an assertion the book does not wholly support—abolition failed to gain approval until 1854. In the meantime, under the legislation of 1821, 1830, and 1848, some 900 slaves received their freedom through the manumission fund, while thousands of slave children, through the free-birth provision, were born into freedom, though this was severely circumscribed until age twenty-five. As a result of these and other (minor) processes, only about 12,000 slaves and 11,000 *manumisos* remained in 1854, the former representing roughly one per cent of the total population. Clearly, slavery was dying as a labor institution, yet it still constituted a substantial capital investment for a few important people. Abolition came when it did because of the disturbed political scene, the incumbent Liberals seeking to deprive the rebellious Conservatives of potential allies among the enslaved blacks.

In addition to this general survey, scholars should appreciate the discussions of bureaucratic problems after 1830, of labor and credit patterns in the coffee sector, and of the conflict between human rights and property rights; the two statistical appendixes (on slavery and exports); and the bibliography.

Still, the book leaves many basic questions unanswered. What were the sex ratio and family structure among slaves? What differences, if any, existed between highland and coastal or urban and rural slavery? What was the political position of the individual slaveowners? What was the political and economic status of freed blacks, and how did that affect the slave system? What connection was there between racial attitudes and slavery?

Lombardi, a competent and enthusiastic young scholar, has made a useful start toward explaining a complex problem in a complex period; much remains to be done.

JOSEPH L. ARBENA
Clemson University

JOHN DUNCAN POWELL. *Political Mobilization of the Venezuelan Peasant*. (Written under the auspices of the Center for International Affairs, Harvard University.) Cambridge, Mass.: Harvard University Press. 1971. Pp. 259. \$8.50.

Latin Americanists have previously discussed

the emergence of the modern Venezuelan political system, but largely without analyzing the role of the Venezuelan peasant in that development. John Duncan Powell has placed the peasant into the total perspective of the Venezuelan political scene of the period from the late 1920s through the 1960s.

Powell contends that the Venezuelan peasantry was incorporated into the political process through an alliance with an urban-based political elite. The repressive dictatorship of Juan Vicente Gómez, coupled with incipient industrialization and urbanization, produced a politically unrepresented and restless urban middle class. At the same time depression conditions of the 1930s and increasing engrossment of the land and commercialization of agriculture by the landholders began to place severe pressures on the Venezuelan peasant. The resultant situation favored mobilization of the peasantry by the new urban-middle elements. An urban-elite-rural-mass alliance was consummated and then effectively challenged the traditional landed-religious-military elite. The urban-peasant alliance served to ensure peasant votes for the urban elite, who, upon achieving power, responded with patronage in the form of agrarian reform benefits. The primary concern of Powell's study was to examine the formation of the alliance and to analyze its subsequent functioning.

The first portion of the monograph traces peasant attempts at unionization, syndicate federation, party participation, and interest articulation. Powell does not presuppose considerable knowledge of recent Venezuelan history on the part of the reader, but skillfully interweaves the peasants' story into a historical narrative describing the urban-elite's struggle to legitimize their authority. Powell, currently an associate professor of political science at Tufts University, brings to this part of the work considerable understanding from previous, extensive research that formed the basis of a thesis, a dissertation, and several articles on topics relating to the Venezuelan peasant. The conclusions are based primarily on official and semiofficial government, party, and union publications, various Venezuelan newspapers, and an exhaustive review of secondary sources.

The remainder of the study is an empirical analysis of the structure and functioning of the

alliance. Powell directs his attention to the central element in the "rural problem-solving system"—the Peasant Federation of Venezuela—and to its linkages with Acción Democrática and other major Venezuelan political parties. He provides concise explanations and accompanying statistical tables that characterize peasant leadership and membership, describe peasant demands and supports, and evaluate governmental responses. Powell concludes by relating the politics of agrarian reform in Venezuela to general theories of political modernization. Data analyzed here were drawn largely from fieldwork that included numerous interviews and a systematic survey conducted in Venezuela in 1961, 1964, and 1965–66, and financed by the Land Tenure Center of the University of Wisconsin and the Inter-American Committee on Agricultural Development.

Powell's critical study of peasant mobilization provides a necessary complement for interpreting the Venezuelan national experience.

EDITH JAMES BLENDON
National Archives

ALFRED STEPAN. *The Military in Politics: Changing Patterns in Brazil*. Princeton: Princeton University Press. 1971. Pp. xiii, 313. \$10.00.

Once more historians are beholden to political science for an excellent work on recent and contemporary history. Although written as a study of comparative politics, Professor Stepan's review and analysis of civil-military relations in Brazil since 1945 provide valuable new information for historians about the Brazilian army and suggest intriguing avenues for research into the political behavior of the military in much of Latin America. The book is divided into four parts, each of which could well be expanded into a separate monograph. Part 1 analyzes the military institution as a subsystem of the national political system. It also confirms the long-held assumption by Brazilianists that the army officer corps is largely of urban, middle-class background and leaves no doubt that career officers are increasingly drawn from military families. Part 2, which examines military and civilian attitudes toward successive military coups after 1945, demonstrates conclusively that both the civilian and military elites looked upon the armed forces as a stabilizing "moderator" of the political sys-

tem during times of crisis. Part 3 considers a range of new factors—including the radicalization of civilian politics and the ideological training of military officers—that persuaded the armed forces to take power in their own name in 1964. Part 4 reviews selected aspects of the record of the military regimes in office between 1964 and 1970. The author questions the view that the armed services are peculiarly qualified to resolve problems of economic development. Here he presents evidence of the widening gap between civilians and military on the one hand and internal divisions within the officer corps on the other. He also asserts, but does not document, the government's increasing reliance upon torture as a means of political control and its lack of a creative program of social development.

Students of Brazilian political history may well dispute some of Professor Stepan's emphases and interpretations, while lay readers may have difficulty following his occasionally unchronological presentation. All in all, however, he has produced a sound and objective study, indispensable for historians concerned with the evolution of the political process in Latin America and highly recommended for those dealing with military governments, past or present, in other areas of the world.

ROLLIE E. POPPINO
*University of California,
Davis*

Mayo documental. Volumes 11 and 12. (Facultad de Filosofía y Letras, Instituto de Historia Argentina "Doctor Emilio Ravignani." Documentos para la historia argentina, Numbers 39 and 40.) Buenos Aires: [the Instituto.] 1965. Pp. 349; 357.

These two volumes continue the fine work of Dr. Ricardo Caillet-Bois and the Facultad de Filosofía y Letras of the Universidad de Bue-

nos Aires. Volume 11 covers the period from January 5, 1810, to June 16. Volume 12 carries on from June 17 to August 10. Each item is numbered and printed in chronological order; each is headed by data in bold-face type; and the heading is reproduced in an index at the back. Altogether it is a highly useful format for student and scholar, though a detailed subject index would have served even better.

Like the previous ten volumes in this series "of ten or twelve volumes," these were a response to the sesquicentennial of Argentine independence, celebrated in 1960. The series has collected documentary material to enlighten the student on the objectives pursued by the European nations in the Rio de la Plata area between 1808 and 1811. The aim goes beyond a study of purely Argentine or Platine history—it considers the events of the Iberian peninsula beginning with the Napoleonic invasion and the deposing of Ferdinand VII, European policies and counterpolicies brought on by the events in France and Spain, and the role of the Rio de la Plata in the history of Europe and the empires of European countries.

These two volumes comprise both unpublished and previously published documents. They are drawn from a wide variety of archives and other sources in South America and Europe. While most would be available to any serious researcher, the time and travel involved would be prohibitive to the individual. They fill a gap in documentary materials not otherwise available to the North American student, who previously had to rely on secondary works. This series provides a fine basis for seminars and research studies in Argentine history of the independence era. It satisfies a long-felt need.

WILLIAM H. JEFFREY
*University of Maine,
Orono*

Communications

A communication will be considered only if it relates to an article or review published in this journal; publication of such a communication or of any part of it is solely at the editors' discretion. Limitations of space require that a communication concerning a review be no longer than the review to which it refers and in no case longer than 500 words. Communications concerning articles or review articles may be no more than 1,000 words, and the editors reserve the right to impose a lower limit. The schedule of publication and the time needed to send a communication to the author of the article or review in question for such reply as he may care to make virtually preclude the possibility of publication in the issue following that in which the original article or review appeared. Unless, in the editors' judgment, some major scholarly purpose is served, rejoinders will not be published.

TO THE EDITOR:

Loren Baritz's review of Charles A. Barker's *American Convictions: Cycles of Public Thought, 1600-1850* (*AHR*, 76 [1971]: 1213-14), is disturbing not because it is critical, but because it is inadequate and hence distorts the character of Barker's study. The substance of Professor Baritz's criticisms does not show why he fails to treat the thesis of this work or its potential importance as a recent contribution to a distinguished body of literature synthesizing the history of American thought, which includes Merle Curti's *Growth of American Thought* (1943), Stow Persons's *American Minds* (1958), Henry Steele Commager's *The American Mind* (1950), and Ralph Henry Gabriel's *Course of American Democratic Thought* (1940). Now Professor Baritz may not approve of efforts at

broad synthesis in the history of ideas or of a master craftsman's desire to reflect publicly upon the fruits of a lifetime of scholarship; but merely ignoring such an effort does not constitute an adequate refutation of its worth in a review article.

We, too, can stop with the preface and conclude that "the book is structured around political thought, reforming religion, economic conceptions, and educational policy." But certainly it is part of the reviewer's portion to read further and to advise his audience of the theme and thesis of the book—the "cycles"—that are neatly and reliably summarized by Professor Barker in the final chapter of *American Convictions*. Professor Barker has attempted to demonstrate the existence of three cycles in American public thought coinciding roughly with the periods 1630-1715, 1715-1800, and 1800-50. Each cycle is given its special identity by its content of four "phases": (1) "the occupation of spacious territories by settlers who had considerable freedom for self-organization—indentured servants and slaves excepted," and the consequent tensions between freedom and organization; (2) "the volunteering of public thought, constructive and critical"; and (3) "widespread rejection of the authority of the national sovereign." A fourth phase, "the phase of reconstruction," is in evidence in the first two cycles. Its elucidation must await Professor Barker's treatment in full of the third cycle, which will be in part the task of volume 2 of *American Convictions*.

Professor Baritz tells us that Professor Barker's "reasons for excluding slavery and the coming of the Civil War are mostly persuasive," but that "there is a flaw in the tone of the argument." The reader ought to be told what that flaw is, since Baritz would make the

charge, and what those reasons are; if he were, he would know that *American Convictions* is volume 1 of a projected two-volume study. Thus Professor Barker's concentration "on matters away from the gathering national fire" is a result of a professional judgment, and, if questioned, should be questioned as such and not dismissed as blandness and complacency.

Some of Loren Baritz's specific criticisms strike me as wholly insignificant. The fact that Barker devotes two pages "to an explication of Jonathan Edwards's *Freedom of the Will*" is not in itself any measure of superficiality in his treatment of Edwards; the number of pages is rarely an accurate measure of profundity. Furthermore, Professor Baritz's objection to Dr. Barker's statement "it is an open secret that the decision for American independence from England in 1776 owes much to the intellectual movement we seek" (p. 191), that "the debate on this interesting question centers around how much is much," is truly exemplary of scholastic quibbling.

Finally, the innuendo indicated by the reviewer's seizure of Barker's reference to Francis Wright as "beautiful but outspoken," and his charge that this phrase is "a cover for a set of political and cultural assumptions," seems calculated to appeal more to the radi-fem-lib contingent than to the disinterested reader. Professor Barker's desire to render his survey a constructive commentary on the past of American convictions should not be so eagerly misconstrued. If the reviewer wishes us to believe that Professor Barker is actually a nasty Burkean, that his "to be sure, the female half of the population, and all but a very few Negroes, were excluded from voting," means that Barker viewed such circumstances with approval, the reviewer should marshal his evidence and announce his verdict openly. Besides, until the presence of reformist zeal is widely acclaimed as an important standard for a historian's achievement, this kind of criticism is probably out of place.

SYLVIA DOUGHTY FRIES

Southern Methodist University

TO THE EDITOR:

Permit me to add a few remarks to the review of Gabriel Jackson's *Historian's Quest* by Joan

Connelly Ullman (*AHR*, 76 [1971]: 1125-26). Professor Ullman deals with what undoubtedly is the core of the book—Jackson's interviews with Spanish participants in the Republic and the Civil War—and she comes to the conclusion that his is a forthright account of the way he personally resolved conflicting testimonies. She rightly states that his account of the interviews provides "the most insight into Jackson's historical perspective and methodology." I would like to point out, however, that Jackson's aim is more far reaching. He wants to show how he became committed to the investigation of problems of the Spanish Republic and how he shouldered his task, becoming ever more aware of its wider context. Apparently the report about his evolution as a historian was written largely as a kind of paradigm. He states in his preface that he hopes to give courage to other people "by discussing the nature of my involvement and my increasing commitment to the work of a historian." This aspect, it seems to me, has been neglected in the review, perhaps unintentionally. Jackson's courageous, straightforward narrative has given me new confidence in the discipline. Young historians, within as well as outside the field of Spanish history, ought to be encouraged to read this book, which tells them—better than any general analysis of methods and aims—why being a historian is still worthwhile.

DIETRICH GERHARD

*Washington University,
St. Louis*

TO THE EDITOR:

Professor Edward Pessen's original, thorough, and enthusiastic article, "The Egalitarian Myth and the American Social Reality: Wealth, Mobility, and Equality in the 'Era of the Common Man'" (*AHR*, 76 [1971]: 989-1034), is flawed by a number of errors and misconceptions concerning top British wealthholders of the nineteenth century. My own work in this area (see my forthcoming article, "Occupations Among British Millionaires, 1857-1969," in the *Review of Income and Wealth*) makes use of the data recorded by the Principal Probate Registry at Somerset House in London, where, since 1857, all estates (including intestacies) probated in

Britain have been valued in a manner that is, apparently, far more comprehensive than for probated American estates. However, until 1926 settled property in which only a life interest was held was not included in the Somerset House reckonings of wealth. Thus, the fortunes of the British landowners are not included in the Somerset House figures. Fortunately, it is possible to compensate for this omission by making use of the figures for gross annual rental in the *Return of Owners of Land* of 1871–76 (subsequently collated by John Bateman, *The Great Landowners of Great Britain and Ireland* [4th ed.; 1883]).

Professor Pessen states on page 1001 that “only a handful of the three hundred or so families constituting England’s landed aristocracy could approximate such wealth [as the duke of Bedford’s \$10,000,000], with others such as the earl of Clarendon worth closer to \$375,000. Sir Francis Baring, whose fortune may have been matched or exceeded in England in the entire nineteenth century by Queen Victoria, Sir Robert Peel, the dukes of Bedford, Northumberland, Bridgewater, and Devonshire, and hardly any others, has been judged to have been worth slightly less than \$5,000,000 during the period. . . .” This statement is mistaken in a number of ways. In the first place, far more than “only a handful” of Britain’s landed aristocracy could approximate the wealth of Sir Francis Baring. Baring’s fortune was slightly more than £1 million. Taking Professor Spring’s formula, which Professor Pessen adopts, of the total estate value being equal to thirty times the gross annual rental minus indebtedness (see page 1004, footnote 30), a gross annual rental of £33,333 is the equivalent—if indebtedness is put to one side—of a fortune totaling £1 million. Bateman lists 161 landowners possessing a gross annual rental of £33,333 or more, and it is well known that the *Return of Owners of Land* failed to include several important categories of landed wealth, for example, all London holdings and certain mineral incomes. Even allowing for the factor of indebtedness, it is clear that there were a substantial number of landed millionaires in nineteenth-century Britain. To single out Lord Clarendon’s \$375,000 is thus quite misleading, for Clarendon was almost singularly poor for so old a family with so high a rank in the peerage. In any case, I

do not understand how Professor Pessen arrives at this value—Clarendon’s annual rental was only £3,700 per annum.

Professor Pessen is also mistaken about the wealth of Sir Robert Peel. His personalty at the time of his death amounted to about £500,000 (Charles Mackay, *Life and Times of Sir Robert Peel*, 4: 641 [London, 1851]), while the lands possessed by his son were worth, according to the *Return*, about £24,500 per annum. It is further surprising that Professor Pessen fails to mention the man who was by all odds the wealthiest Englishman of the nineteenth century, the first duke of Westminster (d. 1899), whose worth was commonly estimated at £13.5 million at the time of his death.

Aside from the great landowners, there were many non-landed millionaires whose wealth is recorded by the Somerset House figures. Between 1857 and 1899, sixty-two male Englishmen left fortunes exceeding £1 million in non-landed fields. Many, including those who died at the beginning of the Somerset House period, were wealthier than Baring: for example, James Morrison (d. 1857), a draper and London property owner, who left £3.4 million; Lewis Loyd (d. 1858), father of Lord Overstone, worth £3.0 million; Thomas Brassey (d. 1870), the railway contractor, worth £3.2 million; and Giles Loder (d. 1871), a Russia merchant, worth £3.0 million. I have not traced the Somerset House sources below the £1 million mark for its earliest years, but it is likely that perhaps a dozen or more persons died in Britain each year leaving non-landed fortunes that would have made them dollar millionaires. The number of living Englishmen who held dollar-millionaire fortunes was probably far in excess of the number of Americans who did so in this period, even if only the non-landed wealthholders are considered.

Professor Pessen finds that “only about two per cent of the Jacksonian era’s urban economic elite appear to have been born poor” (p. 1012). Of the sixty-two non-landed British millionaires deceased between 1857 and 1899, I find that ten could be described as “self-made men” and four placed in an “ambiguous” category, midway between “self-made” and not “self-made.” Men like Thomas Cubitt, the famous London builder, Sir Isaac Holden, a former miner who invented the wool-combing machine, or Joseph

Love, a former pitboy turned colliery owner, were all born to poverty in Britain and died as sterling millionaires. It is clear that something is very wrong here, for evidence that British social mobility is much greater than America's is contrary to all contemporary evidence and historical judgment. Of course, this could be the case but it would be surprising, to say the least. Furthermore, it must not be forgotten that below the millionaire level the percentage of self-made men must have been even greater, for presumably it is easier to acquire, say, £200,000 in the course of a single lifetime than £1 million. Without either disparaging Professor Pessen's evidence or stubbornly asserting the validity of my own, it may be that Professor Pessen, in studying the elites of four older Eastern cities, has gotten hold of those areas least open to new men. It may also be that the Jacksonian era—a sort of trough between the fortune building of the post-Revolutionary era and the American Industrial Revolution—is uniquely restrictive in this respect. After all, the forebears of the Jacksonian wealthholders must have been new men themselves at some time in the past.

WILLIAM D. RUBINSTEIN

Johns Hopkins University

PROFESSOR PESSEN REPLIES:

I believe my article is innocent of the flaws ascribed to it by Mr. Rubinstein. He states that I was mistaken in suggesting that "only a handful" of Englishmen could approximate Sir Francis Baring's wealth, when in fact I made that suggestion not about Baring's fortune but about the duke of Bedford's far greater estate. Mr. Rubinstein cites Bateman's *Great Landowners* as proof that 161 landowners each had landed fortunes of at least £1 million in 1872–73. Mr. Rubinstein appears unmindful that the period I investigated was confined to the decades prior to 1850. Since my cautious estimates of the scope of England's greatest landed estates were based largely on F. M. L. Thompson's *English Landed Society in the Nineteenth Century* (1963), I was of course aware of Thompson's reliance on the *Modern Domesday Book* as "culled" by Bateman. True, the latter source did not include all landed income. But it is also true that it omitted the indebtedness of the great landowners. Bateman himself suggested

that quite typically when a large landed income in *Modern Domesday* was subjected to analysis for mortgage and other charges, the landowner would be left with slightly more than one-fifth the gross amount "to live upon" (*Great Landowners*, 4th ed.; xxiii–xxv). More important, however, is the fact that the Bateman evidence concerns fortunes held a quarter of a century after the close of the period I examined. In a letter he sent me on November 11 apprising me of his intention to publish the above communication, Mr. Rubinstein himself concedes that after the Civil War, "American wealth-holders were so much richer than British wealth-holders (there must have been several dozen [Americans] who possessed more than the Duke of Westminster's reputed £13.5 million). . . ." Bateman's evidence indicates that no more than two dozen or so British landowners held estates whose gross value was greater than £2.25 million. Mr. Rubinstein's references to many great non-landed English fortunes are similarly inappropriate since the fortunes in question refer to the years after 1857. The latter year may seem to be very close in time to the mid-1840s, and fortunes accumulated by the former date may therefore be considered comparable to those achieved a decade earlier. Yet twelve years could make a great difference in the magnitude of fortunes. A comparison of assessed wealth of 1856–57 (as disclosed in the reliable—but excessively modest because incomplete—figures in William H. Boyd, comp., *Boyd's New York City Tax-Book, 1856 & '57* [1857]) with the assessments for 1845 discloses that most of the five hundred richest New Yorkers at "the beginning of the Somerset House period" had more than doubled their fortunes during the previous decade.

Mr. Rubinstein's claim that more Englishmen than Americans were dollar millionaires "in this period" (which period?) may be interpreted by the unwary as a refutation of something I had written. Although long ago I argued that my mother made better apple pies than other boys' mothers, I have never written that our country had more millionaires than other countries. Nothing written by Mr. Rubinstein appears to challenge what I suggested: that the great antebellum American fortunes approximated those of the greatest European accumulators.

Mr. Rubinstein states that I misleadingly "single out" Lord Clarendon's small fortune (the small annual rental of which Mr. Rubinstein appears to have gotten wrong). My reference to Clarendon was drawn from Thompson, who treated Clarendon's lesser income, as did I, as an example of the "wide differences of wealth and resources" among the landed aristocracy (Thompson, *English Landed Society* [1963]). Mr. Rubinstein also asserts that I am mistaken about the wealth of Sir Robert Peel. Since my reading of a number of authorities failed to disclose Peel's precise wealth either to confirm or refute the many contemporary assertions I had read about Sir Robert's great fortune, I refrained from an estimate of it, venturing only the conditional suggestion that Peel's wealth may have matched Baring's. Writing one year after Peel's death, Charles Mackay, while referring to the estimated value of Peel's personal estate, made no evaluation of the worth of his great landed estates. According to Whately Cooke Taylor, Peel's father had in 1830 left a personal fortune of unprecedented magnitude, with Peel receiving by far the largest share in addition to the great estates at Drayton Park, in Staffordshire, and in Warwickshire (Cooke Taylor, *The Life and Times of Sir Robert Peel* [London, 1846-50], 1: 35-36). According to Guizot and to such scholars as Norman Gash and J. R. Thursfield, the Peel family "was one of the wealthiest in England"; F. C. Montague has held that Peel's great personal integrity might be easy to a man in possession of such a "vast fortune" (*Life of Sir Robert Peel* [1889]). As for my "surprising omission" of the late nineteenth-century accumulator, the first duke of Westminster, Rubinstein concedes that many

Americans contemporary with the duke were wealthier than he.

Mr. Rubinstein finds "something wrong" in the contrast between my evidence that few wealthy Americans of the antebellum decades were born poor and his evidence on the backgrounds of sixty-two British capitalists of the late nineteenth century. According to his own account, three of the latter were born poor. That fourteen of them were "self-made" or partly "self-made" does not suggest poverty at their birth. Robert Peel's father, for example, described by Gash as partly a "self-made man," was the son of "Parsley Peel," who left a fortune "not far short of £140,000" (Gash, *Mr. Secretary Peel: The Life of Sir Robert Peel* [1961]). And in attempting even a brief analysis of the parental backgrounds of the British rich, why does Mr. Rubinstein exclude the great landholders who were so substantial a component of the rich and who were "in the main" composed of families long wealthy (Thompson, *English Landed Society* [1963])?

Mr. Rubinstein's surmise that the Jacksonian era was "a sort of trough," while indicating his acceptance of my conclusion that the period was not egalitarian, testifies also to his evident unawareness of the scholarship of William Miller, Frances Gregory, and Irene Neu, among others, who have shown that the American rich of 1870 and 1900 were typically well born.

I must agree, however, with Mr. Rubinstein's final suggestion. The forebears of all of us wealthholders, of whatever era, must indeed "have been new men themselves at some time in the past" after Adam delved and Eve span.

EDWARD PESSEN

City University of New York

Recent Deaths

JOHN RICKARDS BETTS, professor of history at Boston College, died suddenly on August 9, 1971, in Newton, Massachusetts. Born in Bloomsbury, New Jersey, in 1917 and educated in his early years in Easton, Pennsylvania, he graduated from Princeton University in the class of 1938. His subsequent graduate studies at Columbia University, where he received his M.A. in 1939, were interrupted by the Second World War. During army service in Europe, he was awarded the Bronze Star for bravery at Leipzig. In 1951, after returning to civilian life, he received his Ph.D. from Columbia.

His principal research interest was in the history of American sport, an interest that led to the publication of a number of articles in that field as well as on other topics over a wide range of social history. At the time of his death, he was close to the completion of a book to be entitled *Our Sporting Heritage: A Social and Cultural History*, and he had finished research for two projected biographies, one on P. T. Barnum and the other on Dr. Thomas Dwight. Also completed, but not yet published, were three articles on various aspects of nineteenth-century American views and policies concerning public health and facilities for exercise.

His first college teaching was as an instructor at South Dakota State College in 1946. Between 1948 and 1954 he taught at Tulane University. For part of that time he was a member of the staff of the then *Mississippi Valley Historical Review*. In 1954, he joined the Boston College department of history and thereafter was intimately associated with the rapid growth of the department. At various times at Boston College he served as the director of its Institute of American Studies and of its graduate program in American Studies. He played an active role in building the Boston College chapter of the

Association of American University Professors and served as chapter president and as vice-president of the Massachusetts State Conference of A.A.U.P. Chapters. He held the presidency as well as other offices of the New England American Studies Association. His many involvements in political and community affairs included membership on the state board and executive council of the Massachusetts Americans for Democratic Action, the World Affairs Council, the Democratic City Committee of Newton, Massachusetts, and the Catholic Interracial Council of Greater Boston. Among his awards were a Fulbright Lectureship, a Ford Faculty Fellowship, and a Boston College Faculty Fellowship.

His premature death leaves his colleagues and students greatly in his debt, for when they asked, he cheerfully and unstintingly shared with them his wide, rich knowledge of the sources for American social and intellectual history. They will recall particularly his resourcefulness as a director of research, his quiet, unassuming wisdom about man's past and present enthusiasms and anxieties, his courage in the midst of occasional great discouragement, and his modesty in success. As long as they live, those who knew him well will cherish the warmest memories of him as a fine scholar and teacher deeply concerned for the academic and public good as he saw it yet thoroughly tolerant of the views of those who disagreed with him and, in all respects, an unfailingly kind and just man. The death of his wife, Cecelia, after a long illness had preceded his own by only a few months. He left behind a family of five, whose ages range from eight to twenty-four.

WILLIAM M. DALY
Boston College

RUDOLF WITTKOWER, one of the outstanding art historians of our time, died suddenly at the age of seventy on October 11, 1971, in New York City. Wittkower had settled in the United States as recently as 1955 after a distinguished career as a scholar and teacher, first in Italy, and then in England. At Columbia University he built the department of art history and archeology into a large and excellent center for graduate study where, as chairman from 1956 until 1968, he inspired hundreds of students and colleagues and became an acknowledged leader in the American university world.

Wittkower was a pioneer in the study of sixteenth- and seventeenth-century Italian art and of eighteenth-century English architecture. The catalog of the drawings of Gianlorenzo Bernini he and Heinrich Brauer published in 1931 opened a new era of baroque studies. Although sculpture led him into the study of Bernini (his excellent catalog of Bernini's sculpture was published in 1955), he quickly became enmeshed in problems of Italian architecture. Major studies of Michelangelo (1933 and 1934), Carlo Rainaldi (1937), Alberti (1940-41), and Palladio (1944) established Wittkower as the leading architectural historian of his day. His revolutionary book, *Architectural Principles in the Age of Humanism*, first published in 1949, went back to the sources in order to discover the religious and proportional theories on which the practice of Renaissance architecture was based. His catalog of Carracci drawings (1952) opened a new field, and his position as the pre-eminent scholar of the Italian baroque was consolidated with *Art and Architecture in Italy 1600-1750* (1958). This staggering work of synthesis nevertheless includes comprehensive and often novel chapters on the artists whom Wittkower considered outstanding: Bernini, Borromini, Pietro da Cortona, and the architects of Piedmont.

Wittkower was born in Berlin on June 22, 1901, earned his Ph.D. there in 1923, and in the same year settled in Rome as assistant at the art-historical library, the Bibliotheca Hertziana. In 1933 he moved with his wife and son to

London where, in 1934, he joined the newly arrived Warburg Institute of which he became a leading member and exponent. In 1937 he launched the *Journal of the Warburg Institute* as co-editor, a position he retained until 1956. The establishment of the institute in the University of London led, after the war, to university positions; in 1949 he became Durning Lawrence Professor, one of two art-historical chairs in England.

During the war Wittkower turned his attention to local art, particularly Palladianism and neo-Palladianism, in studies that showed the way for a new and positive assessment of English architecture in an international context. *The Earl of Burlington and William Kent* (1948) and *The History of the York Assembly Rooms* (1952) show the focus of these interests, which increasingly settled on eighteenth-century problems. The range, if not the depth and originality of this remarkable scholar's production, can be assayed with the help of the bibliography printed in his two-volume *Festschrift* of 1967 (a supplementary bibliography and a more detailed obituary notice are printed in the *Burlington Magazine* for February 1972 and the *Art Journal*, volume 40).

Although Wittkower's scholarship was unique in its range, vigor, and synthetic ability, his importance can only be dimly perceived by reading the foregoing words. His outstanding quality was his character, which was generous and sympathetic. He inspired even the dullest of us to attempt the impossible—emulation of his own achievement. Wittkower's death marks the end of an art-historical era, not least because of the kindly humanity of the man himself.

Contributions to the Memorial Fund may be sent to Columbia University, New York, New York 10027.

HOWARD HIBBARD

Columbia University

Another member of the association who has died recently is J. G. McAkie of Colorado Mountain College, Glenwood Springs, Colorado.

Festschriften and Miscellanies

These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other *Festschriften* and similar volumes that are amenable to reviewing will be found in the review section.

CHAROVA, KROUMKA, *et al.*, editors. *Études historiques à l'occasion du XIII^e Congrès International des Sciences Historiques—Moscou, août 1970*. Volume V. (Académie Bulgare des Sciences, Institut d'Histoire.) Sofia: the Académie. 1970. Pp. 689. 8.50 L.

Rapports: DIMITAR ANGELOV, CHRISTO DANOV, and VELIZAR VELKOV, Über einige Probleme der sozial-ökonomischen und ethnischen Entwicklung im II.–V. Jh. und des Übergangs von der Antike zum Mittelalter im VI.–X. Jh. DIMITAR KOSEV, Les rapports agraires et le mouvement paysan en Bulgarie de la fin du XVIII^e siècle à nos jours. CHRISTO CHRISTOV, Mouvements idéologiques en Europe du Sud-Est au cours des XIX^e et XX^e siècles (Jusqu'à la Première guerre mondiale).

Études à l'occasion du centenaire du V. I. Lénine: ZH. NATAN, [Vliianie Leninskikh idei na razvitie bolgarskoi istoricheskoi nauki.] v. KHADZHINIKOLOV, [Leninskii zapyt sotsialisticheskogo stroitel'stva i ikh osushchestvlenie v NR Bolgarii.] CÁNCHO VASSILEV, Lenin's Methodological Approach to the Historical Process.

Études de l'Antiquité et du Moyen Âge: ALEXANDRE FOL, Parallèles Thraco-Bithyniens de l'époque pré-romaine. I. Le Régime de la propriété. BORISLAV PRIMOV, Certain Aspects of the International Importance of the First Bulgarian Empire. GENOVEVA CANKOVA-PETKOVA, L'établissement des slaves et protobulgares en Bulgarie du Nord-Est actuelle et le sort de certaines villes riveraines du Danube. PETER KOLEDAROV, West Black Sea Coast Ports in the Late Middle Ages (14th–16th Centuries) Listed on Nautical Charts. G. NESHEV, [K voprosu o sostoiianii bolgarskikh tserkvei i monastyrei v pervye stoletiiia osmanskogo vladychestva.] BISTRA CVETKOVA, Changements

intervenues dans la condition de la population des terres bulgares (depuis la fin du XVI^e jusqu'au milieu du XVIII^e s.). CVETANA GEORGIEVA, Organisation et fonctions du corps des janissaires dans les terres bulgares du XVI^e jusqu'au milieu du XVIII^e siècles.

Études sur l'histoire moderne: KROUMKA CHAROVA, Les idées et l'activité révolutionnaires de Luben Karavelov pendant les années 60 du XIX^e s. JONO MITEV, Les pétitions adressées par les Bulgares aux Grandes Puissances après la repression de l'Insurrection d'Avril concernant l'octroi de l'autonomie au peuple bulgare. IA. IOTSOV, [Burzhuaznaia demokratiia v Bolgarii 1879–1923 gg.] ANDREI PANTEV, Bulgaria in Anglo-Russian Relations in 1892–1894. ZWETANA TODOROVA, Aspekte der industriellen Entwicklung Bulgariens vom Ende des 19. Jhs. bis zum Ersten Balkankrieg. ELENA STATELOVA, Sur la question des relations Bulgaro-Turques au cours de la période 1909–1911. SIMÉON DAMIANOV, Les efforts de la France pour gagner la Bulgarie à la cause des Puissances de l'Entente dans la Première guerre mondiale.

Études sur l'histoire contemporaine: ZDRAVKA MIČEVA, Die Diktatur vom 6. Januar 1929 in Jugoslawien und ihr Widerhall in Bulgarien. LJUDMILA ŽHIVKOVA, The Imperialist Contradictions between England and Fascist Germany in the Balkans on the Eve of World War II. VITKA TOŠKOWA, Die deutsche Aggression auf dem Balkan und die Rolle Bulgariens (April–Mai 1941). A. NAKOV, [Antisovetskaia politika bolgarskikh pravitel'stv nakanune Deviatogo sentiabria 1944.] M. ISUSOV, [O sotsnal'voi strukture bolgarskoi derevni posle vtopoi mirovoi voiny.] BORIS MATEEV, The Establishment of the Cooperative System in Agriculture in Bulgaria.

Historiographie: NIKOLAI TODOROV, Les études balkaniques en Bulgarie. VOIN BOZHINOV, Problems of the Bulgarian History in Several Publications of the Institute for Balkan Studies in Salonika.

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LOUIS RICHARD, Une pendeloque gallo-romaine. MICHEL LE MENÉ, La comptabilité de Jacquet du Boyle, marchand d'Angers (1441-1449). YVES DURAND, Cahiers de doléances de la noblesse des gouvernements d'Orléanais, Normandie et Bretagne pour les Etats généraux de 1614. GABRIEL DEBIEN, Trois documents d'Histoire antillaise: I. Les vivres sur une Cafetière de Saint-Domingue (1786-1791); II. Une Rocouerie à la Guyane pendant la Révolution (1789-1798); III. Un Nantais à la chasse des marions en Guyane (octobre-décembre 1808). PHILIPPE BOSSIS, Enquête sur les districts de Cholet et de Beaupréau (1788). MARIUS FAUGERAS, Les Vocations religieuses de femmes dans le diocèse de Nantes au XIX^e siècle (1802-1914). JACQUES FIÉRAIN, Nantes et la Réunion au temps du second Empire: Les origines de la maison d'armement Henri Polo et C^{ie} (1856-1861).

Études historiques 1970 publiées à l'occasion du XIII^e Congrès International des Sciences Historiques par la Commission Nationale des Historiens Hongrois. Volume 1. Budapest: Akadémiai Kiadó. 1970. Pp. 616.

DEZSŐ NEMES, Lenin ist mit uns. LAJOS ELEKES, Connaissance historique—conscience sociale (Quelques tendances de l'historiographie et de la philosophie de l'histoire—leur influence sur la pensée contemporaine). GYÖZÖ EMBER, Über die historische Statistik.

TIBOR WITTMAN, Andean Nations in the Making (Economic Conditions and Independencia). EMMA LEDERER, Feudalism as a Structure and Form of Society. ANTAL BARTHA, Barbarian and Early Feudal Societies on the Fringes of Europe. ZSIGMOND PÁL PACH, The Role of East-Central Europe in International Trade (16th and 17th Centuries). ÁGNES R. VÁRKONYI, Historical Personality, Crisis and Progress in 17th Century Hungary. ISTVÁN SINKOVICS, Die akademische Bildung in Ungarn im 17. Jahrhundert. GYÖRGY SPIRA, Le grand jour (Le 15 mars 1848). ISTVÁN DIÓSZEGI, Die Österreichisch-Ungarische Monarchie in der internationalen Politik im letzten Drittel des 19. Jahrhunderts. TIBOR ERÉNYI, Die Sozialdemokratische Partei Ungarns und die Außenpolitik der Österreichisch-Ungarischen Monarchie in den Jahren 1908-1914. GYULA TOKODY, Der Weg der grossdeutschen Geschichtsschreibung zum Faschismus. JÁNOS JEMNITZ, Reflections on the Symptoms of the Separation of the "Ultras" and the "Majority" Social-Chauvinists (1914-1917). DÉRŐ BORSHANI, [Sotsialisticheskii dvizheniia v Vengrii v period mezhdur dvumia mirovymi voynami.] MIKLÓS LÁTSKO, [K voprosu o fashizme v Iugo-vostochnoi Evrope.] LÁSZLÓ ZSIGMOND, A propos de la discussion sur la notion et l'interprétation de la démocratie chrétienne. SÁNDOR BALOGH, Die Geschichte Ungarns nach dem Zweiten Weltkrieg in der marxistischen Geschichtsliteratur. IVAN T. BEREND and D. PANKI, [Ekonomicheskii rost i izmeneniia ekonomicheskoi struktury v sotsialisticheskikh stranakh Vostochnoi Evropy posle vtoroi mirovoi voyny.]

Other Books Received

Books listed were received by the *AHR* between October 1 and December 1, 1971. Books that will be reviewed are not listed, but listing does not preclude subsequent review.

GENERAL

- BAINTON, ROLAND H. *Women of the Reformation in Germany and Italy*. Minneapolis: Augsburg Publishing House. 1971. Pp. 279. \$7.95.
- BINDER, LEONARD, et al. *Crises and Sequences in Political Development*. No. 7. Princeton: Princeton University Press. 1971. Pp. xi, 326. \$8.00.
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- CHOMSKY, NOAM. *Problems of Knowledge and Freedom: The Russell Lectures*. New York: Pantheon Books. 1971. Pp. xi, 111. \$4.95.
- Colloque sur les problèmes de la Conférence européenne pour la coopération et la sécurité*. 2^e colloque européen. Bucharest: Association de Droit International et de Relations Internationales de la République Socialiste de Roumanie. 1971. Pp. 166.
- COLTHAM, JEANETTE B., in collaboration with JOHN FINES. *Educational Objectives for the Study of History: A Suggested Framework*. Teaching of History Ser., No. 35. [London:] Historical Association. 1971. Pp. 28. 30p.
- COTLOW, LEWIS. *The Twilight of the Primitive*. New York: Macmillan. 1971. Pp. xiii, 257. \$10.00.
- CUGIS, CARLO DE (ed.). *England and Italy a Century Ago: A New Turn in Economic Relations. Appendix to the Catalogue of the Exhibition Held during the British Week in Milan (9-17 December 1965)*. Milan: Banca Commerciale Italiana. 1968. Pp. xvii, 547. See rev. of *England and Italy a Century Ago*, *AHR*, 75 (1969-70): 1102.
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- HOLZER, HANS. *The Aquarian Age: Is There Intelligent Life on Earth?* Indianapolis: Bobbs-Merrill. 1971. Pp. xii, 131. \$5.00.
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- KORSCH, KARL. *Marxism and Philosophy*. Tr. and with an introd. by FRED HALLIDAY. New York: [Monthly Review Press]. 1970. Pp. 175. Cloth \$7.50, paper \$2.95.
- LARSON, ARTHUR D. (comp.). *Civil-Military Relations and Militarism: A Classified Bibliography Covering the United States and Other Nations of the World, with Introductory Notes*. Bibliography Ser., No. 9. Manhattan: Kansas State University Library. 1971. Pp. vii, 113. \$3.00.
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- LUTTWAK, EDWARD. *A Dictionary of Modern War*. New York: Harper and Row. 1971. Pp. 224, 96 plates. \$7.95.
- NOBLE, JOHN. *A Treasury of Beautiful Dolls*. Foreword by DOROTHY, ELIZABETH, and EVELYN COLEMAN. New York: Hawthorn Books. 1971. Pp. xxiii, 210. \$17.50.
- NOCK, O. S. *Railways in the Years of Pre-eminence, 1905-1919*. Railways of the World in Color. [New York:] Macmillan. 1971. Pp. 194. \$3.95.
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WSZOLEK, JAN. *Prawica Wielkiej Emigracji wobec narodowego ruchu włoskiego (Przed Rewolucją 1848 roku)* [Relation of the Polish Right-Wing Emigrés to the Question of the National Italian Movement (Before the Revolution of 1848)]. Polska Akademia Nauk—Oddział w Krakowie. Prace Komisji Nauk Historycznych, No. 26. Cracow: Zakład Narodowy Imienia Ossolińskich, Wydawnictwo Polskiej Akademii Nauk. 1970. Pp. 138. Zł. 25.

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BAK, JANOS M. (comp.), with the cooperation of the Humanities Division. *Guide to Reference Materials in Medieval History in the Libraries of the University of British Columbia, with a Selection of Primary Sources in English Translation*. Reference Publication No. 36. Vancouver: University of British Columbia Library. 1971. Pp. vi, 54. \$1.00.

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BOWSKY, WILLIAM M. (ed.). *The Black Death: A Turning Point in History? European Problem Studies*. New York: Holt, Rinehart and Winston. 1971. Pp. 128.

CAMPBELL, ALISTAIR. *Skaldic Verse and Anglo-Saxon History*. The Dorothea Coke Memorial Lecture in Northern Studies delivered at University College London, 17 March 1970. London: H. K. Lewis for the College. 1971. Pp. 16. 30p.

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BRITISH COMMONWEALTH AND IRELAND

BATEMAN, JOHN. *The Great Landowners of Great Britain and Ireland*. With an introd. by DAVID SPRING. The Victorian Library. Reprint of 4th ed. (1883); New York: Humanities Press. 1971. Pp. 22, xxviii, 533. \$10.50.

BLOWS, R. P. (ed.). *History at the Universities: A Comparative and Analytical Guide to Degree Courses in History in the United Kingdom*. 3d ed.; London: Historical Association. 1971. Pp. 172. 60p.

HALE, SIR MATTHEW. *The History of the Common Law of England*. Ed. and with an introd. by CHARLES M. GRAY. Classics of British Historical Literature. Chicago: University of Chicago Press. 1971. Pp. xxxviii, 173. \$9.00.

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PALTER, ROBERT (ed.). *The Annus Mirabilis of Sir Isaac Newton, 1666-1966*. Cambridge, Mass.: M.I.T. Press. 1970. Pp. viii, 351. \$15.00.

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FRANCE

ALBERT-SAMUEL, COLETTE, et al. (eds.). *Bibliographie annuelle de l'histoire de France du cinquième siècle à 1945: Année 1970*. Comité Français des Sciences Historiques. Paris: Éditions du Centre National de la Recherche Scientifique. 1971. Pp. lxxi, 624. 86 fr.

GODECHOT, JACQUES. *The Counter-Revolution: Doctrine and Action, 1789-1804*. Tr. from the French by SALVATOR ATTANASIO. New York: Howard Fertig. 1971. Pp. x, 405. \$12.95. See rev. of French ed. (1961), *AHR*, 67 (1961-62): 402.

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TULARD, JEAN. *Bibliographie critique des mémoires sur le Consulat et l'Empire écrits ou traduits en français*. Centre de Recherches d'Histoire et de Philologie de la IV^e Section de l'École pratique des Hautes Études. Ser. 5, Hautes études médiévales et modernes, No. 13. Geneva: Librairie Droz. 1971. Pp. xiv, 182.

GERMANY, AUSTRIA, AND SWITZERLAND

ARONSON, THEO. *The Kaisers*. Indianapolis: Bobbs-Merrill. 1971. Pp. xii, 276. \$8.95.

GRANT, NEIL. *Munich: 1938. Appeasement Fails to Bring "Peace for Our Time."* World Focus Book. New York: Franklin Watts. 1971. Pp. 81. \$3.95. Grades 7 up.

HILBERG, RAUL (ed. with commentary). *Documents of Destruction: Germany and Jewry, 1933-1945*. Chicago: Quadrangle Books. 1971. Pp. xii, 242. Cloth \$8.95, paper \$2.95.

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DODGE, NORTON T. (ed.). *Analysis of the USSR's 24th Party Congress and 9th Five-Year Plan*. Proceedings of a symposium sponsored by the Washington Chapter of the American Association for the Advancement of Slavic Studies and the Institute for Sino-Soviet Studies, George Washington University, April 30-May 1, 1971. Mechanicsville, Md.: Cremona Foundation. 1971. Pp. 100. \$2.50.

GOLDSTON, ROBERT. *The Fall of the Winter Palace, November 1917: Old Russia's Tsardom Is Swept Away by Bolshevik Revolution*. World Focus Book. New York: Franklin Watts. 1971. Pp. 66. \$3.95. Grades 7 up.

LUKÁCS, GEORG. *Lenin: A Study on the Unity of His Thought*. Cambridge, Mass.: MIT Press. 1971. Pp. 104. \$1.95.

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SMITH, JESSICA (ed.). *Voices of Tomorrow: The 24th Congress of the Communist Party of the Soviet Union*. New World Review Collection. [New York: NWR Publications.] 1971. Pp. 192. \$1.95.

TREADGOLD, DONALD W. *Twentieth Century Russia*. 3d ed.; Chicago: Rand McNally. 1971. Pp. xiii, 563. \$9.95.

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BHARGAVA, K. D. (ed.). *Descriptive List of Secret Department Records*. Vol. 2, 1776-80. Delhi: Manager of Publications, Government of India, for the National Archives of India. 1960. Pp. xv, 406. \$12.87.

BOTHWELL, JEAN. *The First Book of India*. Rev. ed.; New York: Franklin Watts. 1971. Pp. 81. \$3.75. Grades 4-6.

GOH CHENG TEIK. *The May Thirteenth Incident and Democracy in Malaysia*. Oxford in Asia Current Affairs. New York: Oxford University Press. 1971. Pp. 76. \$2.50.

GOLDSTON, ROBERT. *The Long March, 1934-1935: A Red Army Survives to Bring Communism to China*. World Focus Book. New York: Franklin Watts. 1971. Pp. 66. \$3.95. Grades 7 up.

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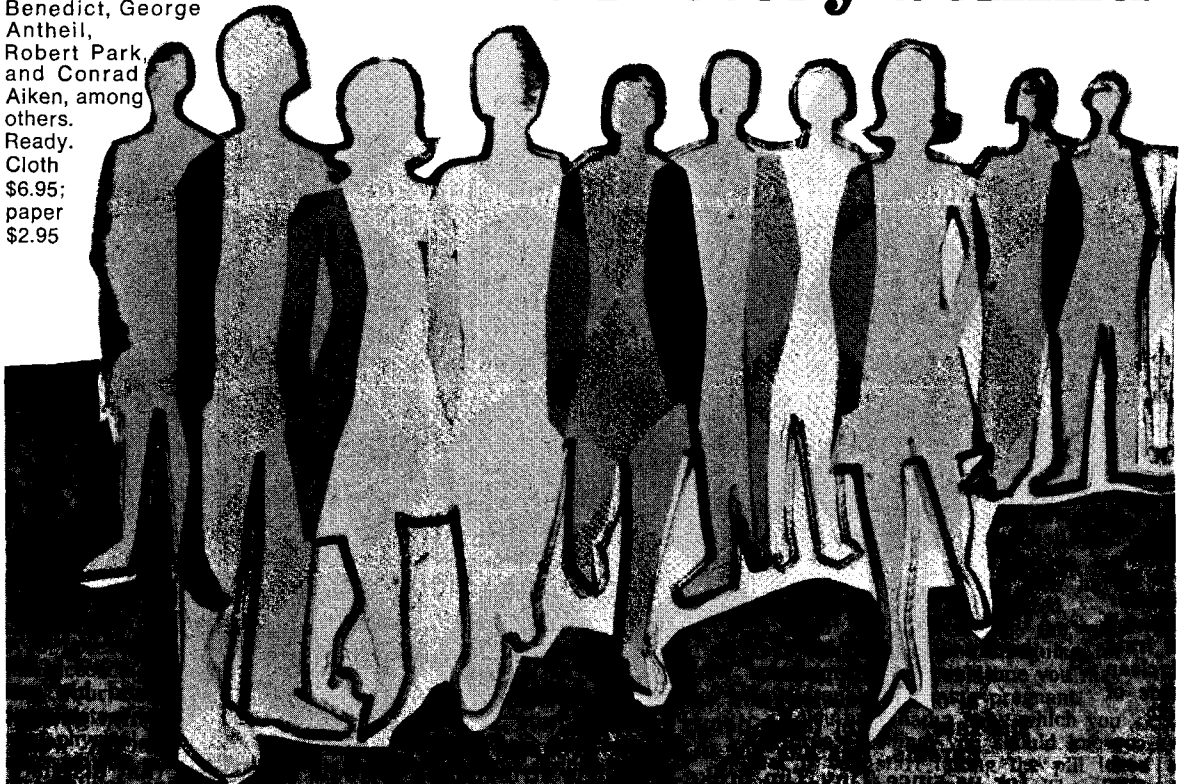
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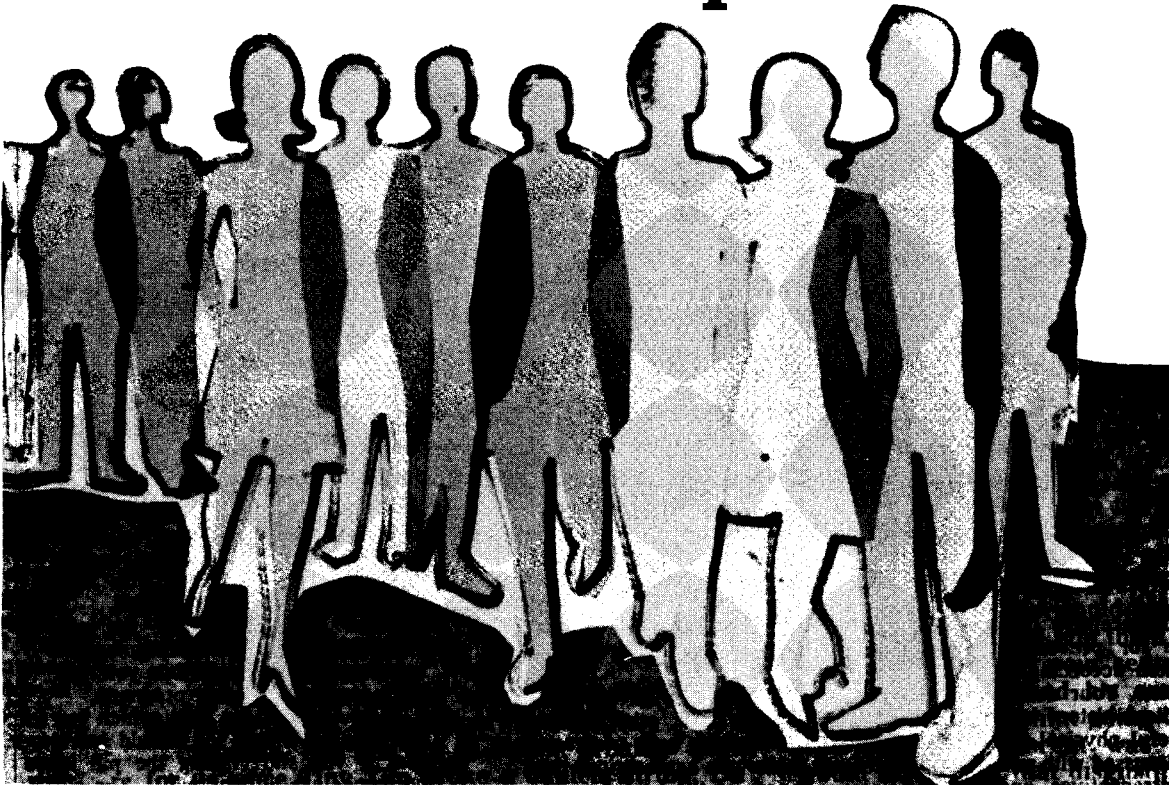
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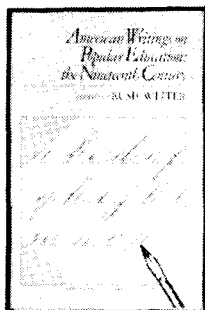
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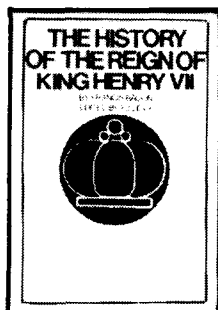
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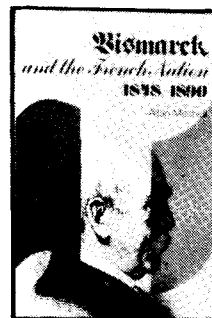
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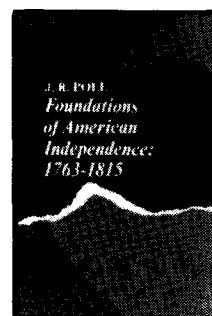


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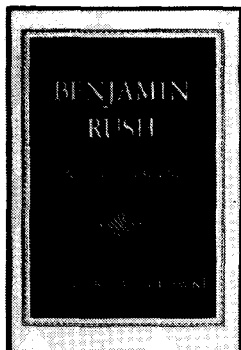
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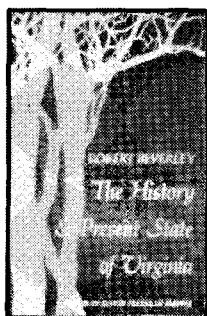
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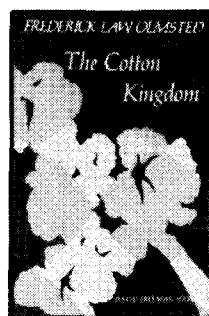
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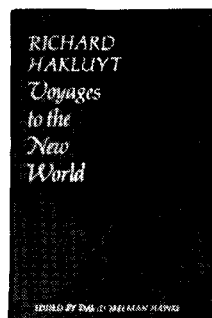
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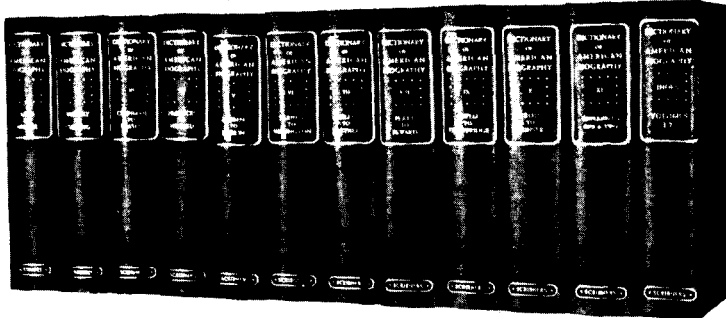
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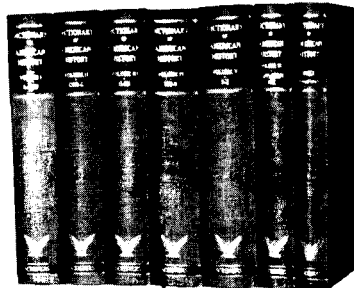
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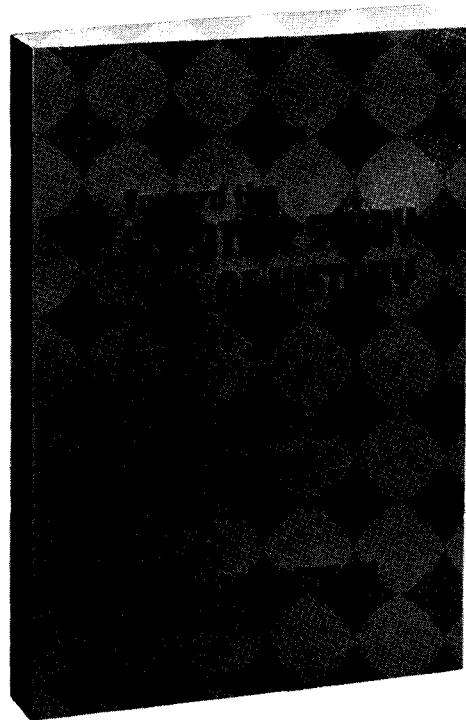
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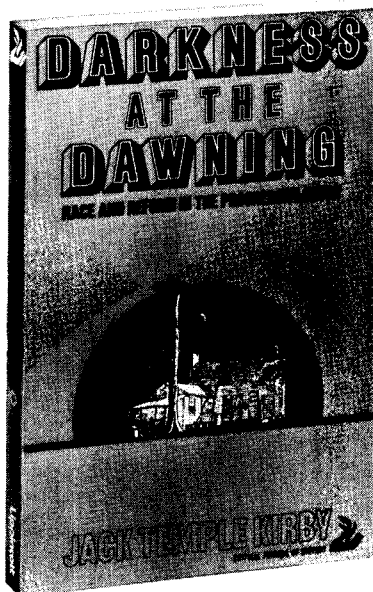
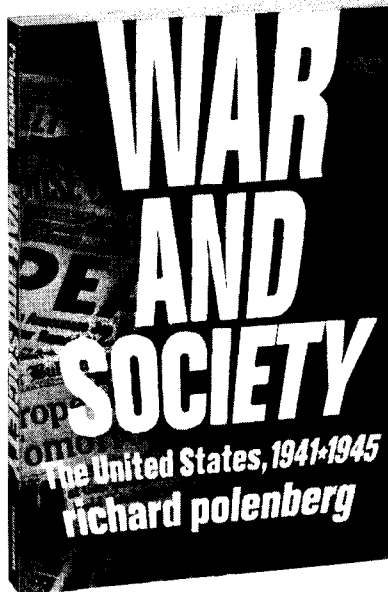
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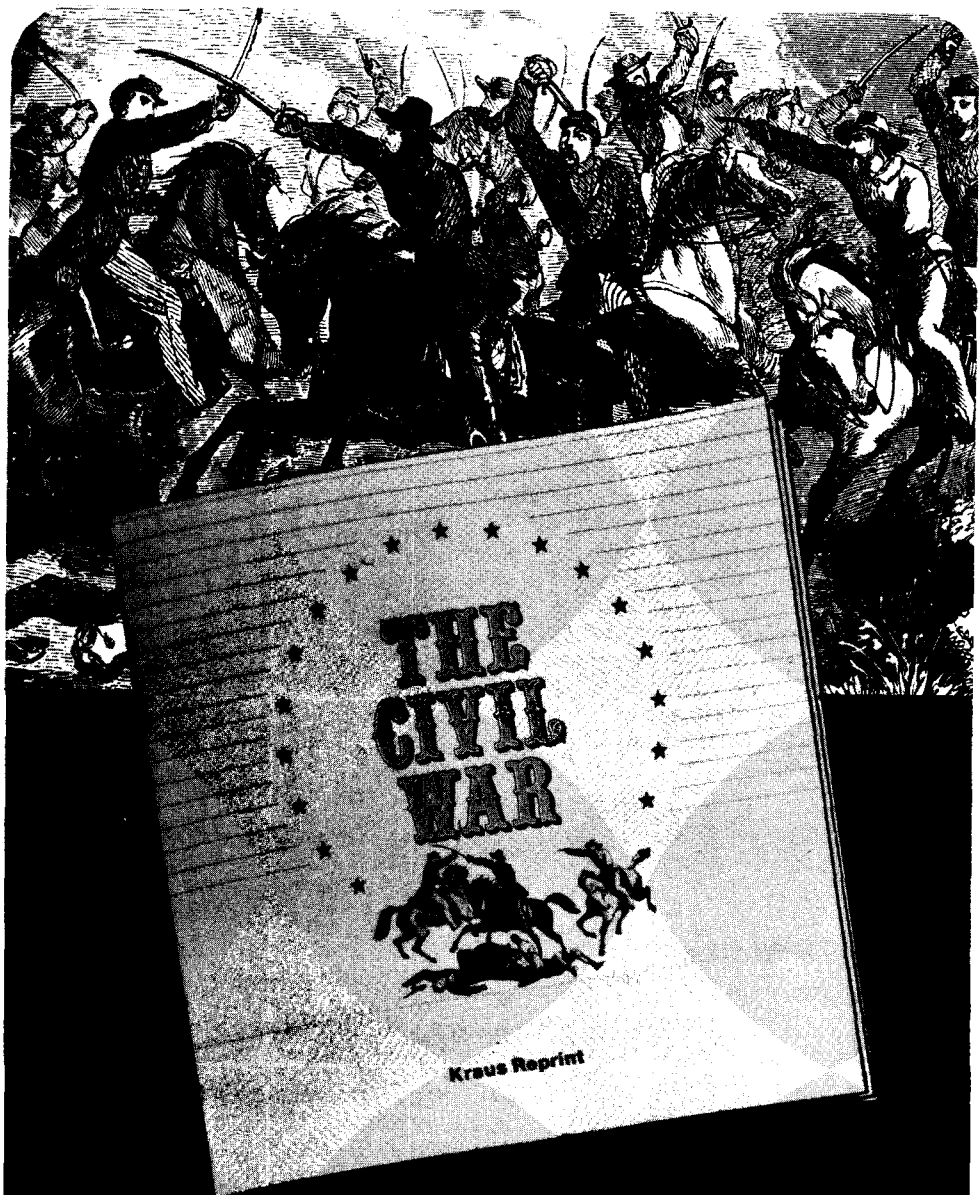
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93 FRANCESCO COPPINI *to the*
DUKE OF MILAN

Borgogna, cart. 514 Orig.

A di passati da Bruggs, essendo tornato d'Ingheterra, scripsi ad la V. Ex. delle cose de là et della cason de nostra venuta; et appresso quanto succedeva et quanto speravo poter seguir, essendo aiutato come V. I. S. havea scripto et datone ferma speranza. Aspecto che sia seguito per la venuta di Messer Antonio de la Torre, el quale venne fornito pienamente de quanto V. I. S. richiedeva. Hora benché le cose d'Anglia habbino havuto più mutatione, non dimeno ad fine Mons. de Varvich é rimaso al di sopra et fatto uno novo Re, quello figlio del Duca Eboracense, Conte de Clemantia, el quale fu insieme cum Varvich ad tornar cum nostri in Anglia; sicché le cose passano in forma che se sono aiutate, come havemo più volte dicto et sollicitato, si farà cosa degna et de maggior gloria che fussi da 500 anni in queste parti, perché costui é giovane, prudente e magnanimo et se vede il caldo del S. P. colla assistentia mia, con reputatione debita, ogni cosa farà degna et grande; alias van in ruina tutti li nostri pensieri et le mie fatiche. Et perché cognosco bene che questo fatto é in mano de V. I. S., ne sto de bona voglia; altrimenti io sarei già in camino al venire. Sicché degnisse la S. V. hora lavorar che é il tempo et aiutar la materia, de la quale per lo passato V. Ex. ha sollicitato, ma che questo é uno tratto maraviglioso d'aconciar lo stato de Italia et lo vostro spetialmente per semprenai. Pensate, S. mio, quanto verisimilmente ve appareccherà la fortuna: i. comodità sifata;

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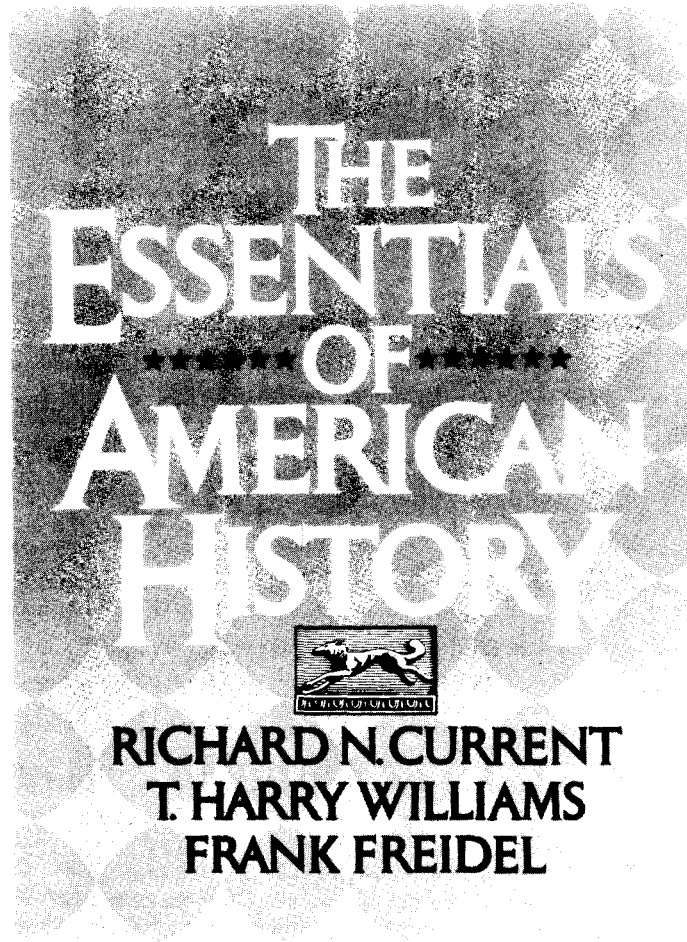
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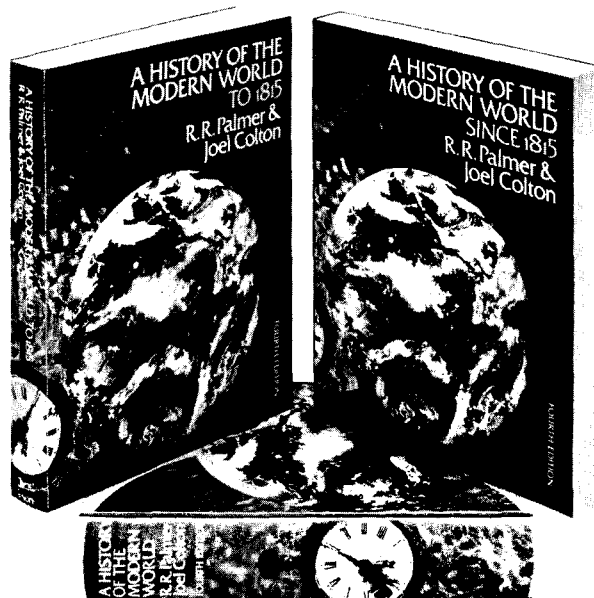


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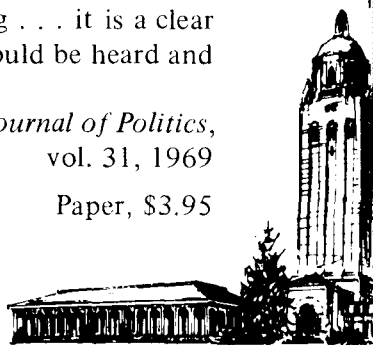
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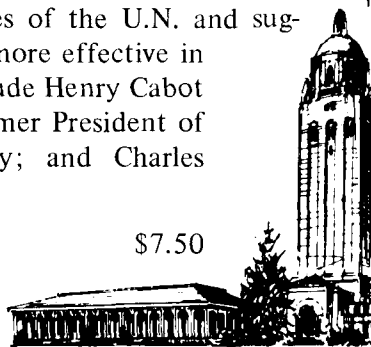
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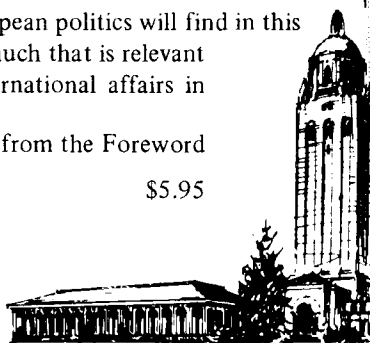
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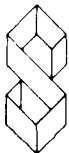
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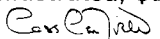
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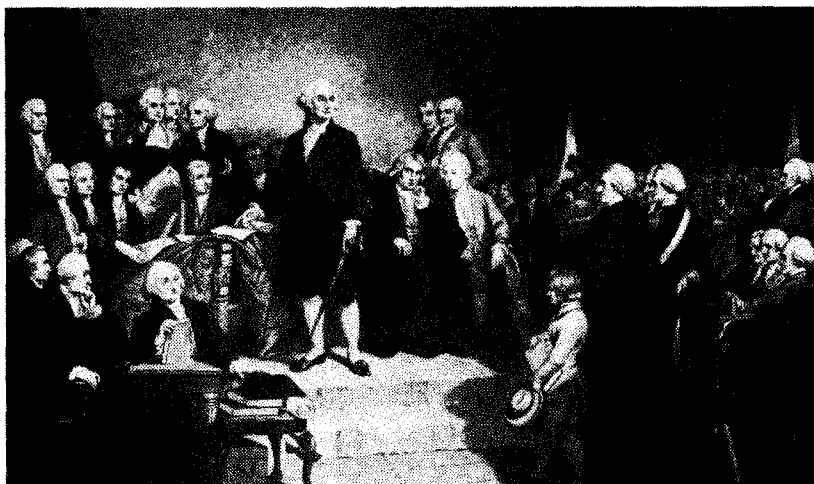
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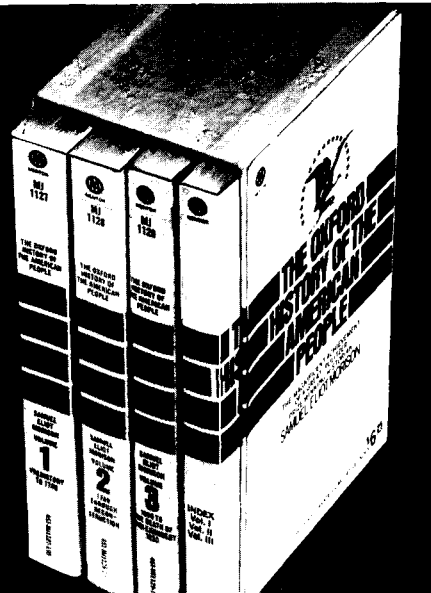
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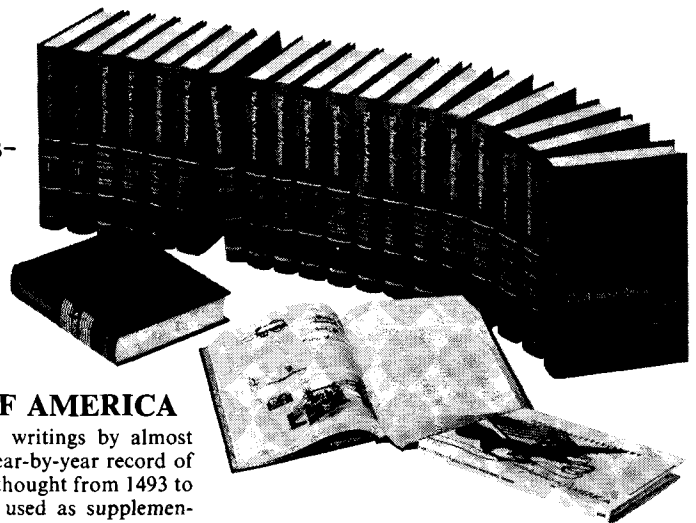
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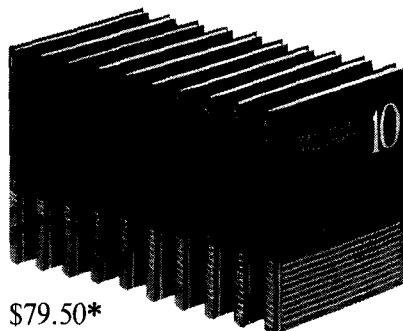
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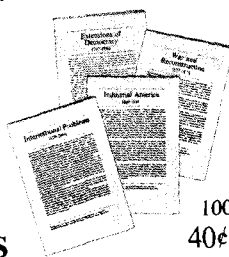


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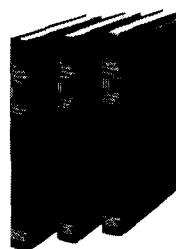
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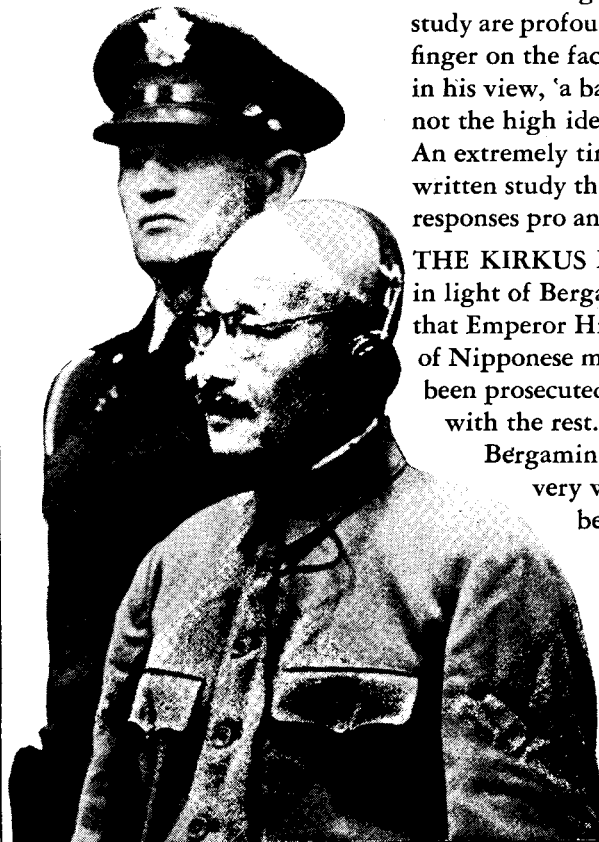
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